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THE
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THE
DUBLIN QUARTERLY JOURNAL
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3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
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6. The Lancet.
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ON A NEW OPERATION FOR THE CURE OF HARE LIP.

By MR. WILLIAM STOKES.

Date of Operation, July 7, 1869.

THE
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AUGUST 1, 1870.

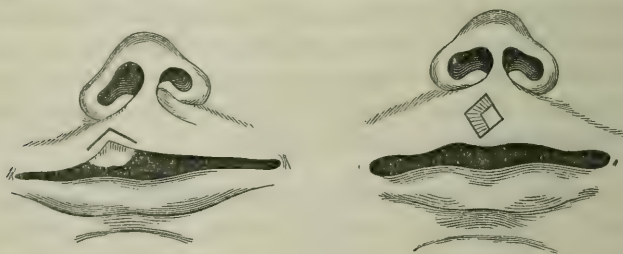
PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*On a New Method of Effectually Remedying the Defect of Hare-Lip.* By WILLIAM STOKES, Jun., M.D.; Surgeon to the Richmond Surgical Hospital; Lecturer on Theoretical and Operative Surgery, Carmichael School of Medicine; Member of the Pathological, Obstetrical, and Surgical Societies of Ireland; Lecturer on Clinical Surgery; Fellow of the Royal Medico-Chirurgical Society of London, &c., &c.

ALL operations hitherto devised and practised for remedying the congenital malformation called hare-lip, present two prime difficulties to the operating surgeon. These are, the getting rid, or at all events diminishing the chances of, the subsequent occurrence of two deformities, which occasionally in truth but too frequently occur, after even the most skilfully performed operation, and for the avoiding of one of which, at least, as is well known, many and various plans have been recommended. The deformities I allude to are—first, the occurrence of a notch on the red border of the lip at the lower termination of the cicatrix, which, though not an invariable, is a most frequent defect; and, secondly, the vertical groove, which in nearly all cases hitherto operated on, exists to a greater or less degree, and which is produced by the contraction or falling in of the tissues at this situation. For this

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defect, when it occurs, there is, I may say, absolutely no remedy. To rectify the first of these, the occurrence of the notch, namely, no insurmountable difficulty presents itself to a surgeon of ordinary ingenuity and dexterity. It is unnecessary to allude to what has been done in this direction by Langenbeck, Malgaigne, Samuel Smith, of Leeds, and many other eminent operating surgeons, as their recommendations, as well as those of other distinguished operators, are, doubtless, already sufficiently well known to all practical surgeons. But I feel confident, that all who are possessed of experience in such cases will bear me out in stating, that the formation of a notch is a sequela which, if possible, should be avoided by all the means in our power; no matter how dexterously and expertly the operation may have been performed, its occurrence is the rule and not the exception. With regard to the second deformity I have spoken of, the vertical groove or sulcus, namely, it is notorious that hitherto no attention has been directed to its avoidance. It has long appeared to me that an operation which would efficiently get rid of the chances of both of these defects supervening, should be devised, and that if such were found possible, there would be then left, in dealing with the deformity of hare-lip, nothing to be desired.



M. Nélaton's operation.

To attain these objects the adoption of the principle of the utilization of the parings appeared to me to be essential. The operations, in which this principle has as yet been either in part or entirely adopted, are those of M. Nélaton, of Samuel Smith, of Leeds, and of my late valued friend and colleague, Mr. Maurice Collis. These procedures, however, ingenious though they be, appear to me to possess certain defects. The principle of M. Nélaton's operation can be best understood by a reference to the accompanying diagram, and the defects in it appear to be that, while it utilizes the parings, it leaves in the ordinary situation of

the notch a projection so great that a subsequent curtailment of it becomes a necessity. Secondly, it does not get rid of the vertical groove caused by the falling in or contraction of the tissues at the line of the cicatrix; and lastly, it is only capable of being adopted in cases where the labial cleft is extremely limited.

Mr. Collis' operation, ingenious though it be, is an extremely complicated procedure, and, in my opinion, possesses two other disadvantages, which are, the necessity of turning up the upper portion of the flap, attached to the ala nasi, which includes the entire thickness of the lip, and fastening it to the upper portion of the mesial flap, which does not comprise all the tissues of the lip. This turning up of the upper portion of the alar flap must obviously be accompanied by great constriction of its vessels, and the part must therefore be very liable to perish; and again, when this does not occur, a puckering at this situation takes place, which is in itself a considerable deformity.



Mr. Collis' operation.

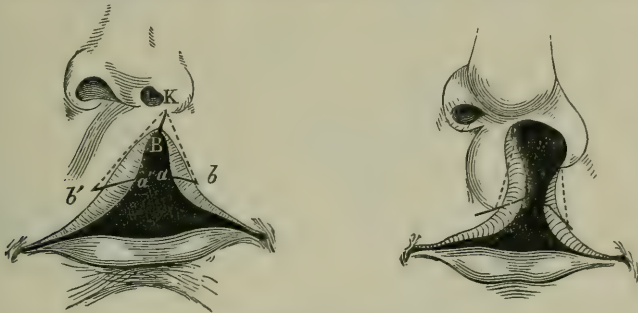
But there can be no doubt that surgeons are much indebted to Mr. M. Collis for having prominently put before them the importance of utilizing the parings in operations for hare-lip. The operation known as Malgaigne's or Sédillot's, Mr. Collis states he "learned twenty years ago from a fine old English surgeon, Mr. Samuel Smith, of Leeds. He told me then that he had devised it some twenty or thirty years previously. Whether he ever published it I do not know. This improvement consists in removing the lower portion of the parings at each side, and turning them downwards so as to form a prominence along the margin."—*Dub. Quart. Jour.*, May, 1868.

The steps of my operation may now be briefly stated. I may mention, in passing, that the complete incisions, or those through

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the entire thickness of the lip, are indicated in the accompanying diagram by black lines; the others, or those which do not go through the entire lip, are indicated by dotted lines. Having either removed the pre-maxillary bone (should it be displaced, which is the plan advocated by Mr. Adams) or placed into its normal situation, which can be done either by pushing it back immediately with a broad pair of forceps slightly curved, the opposing surfaces of the blades being covered with soft chamois leather, or if the projection be too great, by first removing a V-shaped piece of the vomer, as first, I believe, suggested and practised by Professor Langenbeck, of Berlin; or again, by adopting the plan of Boyer, of making for a considerable time previous to the operation a firm and steady pressure on the projecting bone; I then, with a curved scissors, divide all the false frena. I quite agree with the views held by the late Mr. Collis, as to the importance of preserving the true frenum, "its obvious use is to steady the lip, to prevent muscular action from drawing the centre of the lip away from its proper place." Again, he remarks—"The division of the frenum unsteadies the lip, makes it tend to run into a string, and in many cases leaves a permanent fistula into the nostril. It is, moreover, quite unnecessary, as the parts can be brought into perfect apposition without meddling with it." I should mention that previous to any cutting with either scissors or knife, in order to get rid of the great annoyance of hemorrhage, which in young subjects is a matter of grave moment, and in all cases, when it occurs seriously, interferes with the comfort of the operator, I invariably apply at each angle of the mouth one of the largest of the late Mr. Nunneley's artery forceps, and in all cases I have found that it effectually commanded the hemorrhage. Another advantage of these truly practical instruments at this stage of the operation is, that they enable the operator to dispense with the aid of assistants pressing with their fingers the lips in these situations, and of necessity impeding the operator to a greater or less extent. The forceps having been applied at each angle of the mouth, I seize the edge of the cleft at its lower extremity with a double hook, draw the parts forwards, in order to put them on the stretch, and with a fine narrow-bladed knife (Graefe's knife, for the new modified linear extraction operation, or a fine Pollock's knife, answer the purpose remarkably well), I make an incision through the entire thickness of the lip, taking care, however, to stop at about two or three lines above the red

border of the lip. I then make another similar incision on the opposite side. Two *serres fines* must then be applied to the extremities of these flaps, which have the two-fold advantage of arresting any hemorrhage, however slight, that may occur, and also by their weight making the flaps hang down below the red border of the lip, thus keeping them out of the way of the operator until the time comes for uniting them by a point or points of



The Author's operation.

interrupted suture. The two points, *a* and *a'*, can thus be brought down below the red border of the lip, thus forming a projection, and effectually getting rid of the subsequent occurrence of a notch in this situation. The next step of the operation consists in making a vertical incision (*K B*) through the entire thickness of the lip, with a small straight scissors at the upper angle of the cleft. This, of course, is only necessary in cases where the labial cleft does not extend into the nose. The making of the partial incisions at the edge of the cleft on either side, constitute the third stage of the operation, and for these the knives that answer best are either Graefe's cataract knife, a straight iridectomy knife, or Pollock's knife. These incisions are made going through fully two-thirds or three-fourths of the thickness of the lip from the upper point of the vertical incision *K* down to *b* on one side, and from *K* to *b'* on the other. Great care should be taken to prevent the knife going too far and wounding the mucous membrane of the lip. The two quadri-lateral flaps (*K B a b*) and (*K B a' b'*) united behind by the mucous membrane of the posterior aspect of the lip, can then with great facility be turned back, and the broad raw surfaces of each brought into apposition. A tolerably strong well tempered spear-pointed needle should then be introduced from *b* to *b'*. In the great majority of cases one is sufficient. A second may, however, be introduced higher up, and the raw surfaces of

the quadri-lateral flaps approximated by an ordinary figure of eight suture. When, however, a second needle is not deemed necessary, the edges can be approximated and held together by fine entomologist pins, the introduction of which into surgery, from their being so singularly unirritating, is a real boon. For the point of suture below fastening the points *a* and *a'* the fine Chinese suture silk, which can be obtained from M. Lüer, of Paris, can be used with great advantage, as it is, of all the substances I have used for this purpose, the least irritating. Formerly, following the recommendation of Mr. Maurice Collis, I was in the habit of using largely horse hair for interrupted sutures. It is certainly a substance which when introduced into living tissues gives rise to little or no irritation. But notwithstanding this, I am not now as warm an advocate for it as I once was, in consequence of it being occasionally so brittle. For, in truth, I cannot but look on it as a very unfortunate circumstance, having, on a suture breaking, to re-introduce another needle and suture.

As yet I am unable to give more than the particulars of three cases in which I have practised this method of operating; but from the results I have obtained, a good idea of which can be got from the accompanying wood-cuts, I may, I think, fairly claim to have succeeded in attaining the two prime objects I had in view in setting out, which were, to get rid of all chance of the subsequent occurrence of the two defects which, as I stated already, so frequently supervene after the operation for hare-lip; namely, the notch at the red border of the lip and the vertical sulcus or groove corresponding to the line of the cicatrix. The chances of this latter defect occurring are rendered simply impossible, in consequence of the great breadth and thickness of the opposing raw surfaces; being, as they are, double the thickness of any other portion of the lip. The plan which I have adopted for preventing the formation of a notch also gets rid of the chance of that defect subsequently occurring. By operating in this way, therefore, we get rid of all the defects of the other procedures, in which, in part or entirely, the principle of the utilization of parings has been adopted. For—

First—No subsequent curtailment of the projection at the lower extremity of the cleft is necessary.

Second—The procedure is applicable to all forms and varieties of hare-lip.

Third—There is no chance of portions of the soft tissues perishing from any twisting of them.

Fourth—There can be no subsequent puckering.



ON A NEW OPERATION FOR THE CURE OF HARE LIP.

By MR. WILLIAM STOKES.

Date of Operation, September 9, 1869.

Fifth—There can be no subsequent notch.

Sixth—The possibility of any vertical sulcus or groove in the line of the cicatrix is also prevented, which is the case after Sédillot's, Malgaigne's, Samuel Smith's, and, in truth, all other operations hitherto devised for remedying the defect of hare-lip.

Before concluding, there is one other topic connected with the operation of hare-lip to which I wish to draw attention. I allude, namely, to the time which is best for the performance of operations for remedying this defect. As regards this point, surgeons are much divided in opinion; some eminent operators maintaining that where it is possible all such operations should be performed before dentition, while others hold that better results are obtained in all cases when the procedure has been deferred until a later period. My experience would lead me to adopt the views of the latter class, and my reasons for doing so are—First, that the structures at and near the situation of the operation being, at the period of life previous to dentition, so weak, soft, and pulpy, if I might so express myself, are ill able to bear the continuous pressure of the needles and sutures, and that consequently they are specially liable to the occurrence of ulceration and the subsequent formation of transverse cicatrices, which, in some cases, when they occur, are permanent deformities, and may be said to be almost irremediable. Another reason for my performing the late operation is, that the hemorrhage which, even though it be extremely slight in amount, is in very young subjects fraught with some danger. And lastly, there is the much greater difficulty, owing to the smallness of the parts, of adopting any of the plans that have been devised for the avoidance of the defects which sometimes subsequently occur, and which I have already spoken of. These constitute the principal reasons for my preferring the late operation to the one performed previous to dentition. In support of this opinion, I have, I am glad to say, the high authority of my colleague in the Richmond Hospital, Mr. Adams. Several eminent dental surgeons of this city hold, I am aware, the opinion that the early operation is, for many reasons, preferable; but on the other hand, I have been informed by most trustworthy authority, that Mr. Samuel Maclean, whose death some years ago was a subject of the deepest regret to all who knew him, but whose reputation did not die with him, but lives still, was a warm advocate for the late operation. He went so far as to state that, if possible, in all cases operation should be deferred until after second dentition.

ART. II.—*Observations on Fractures of the Sternal Extremity of the Clavicle.* By ROBERT W. SMITH, M.D., Professor of Surgery in the University of Dublin, Surgeon to the Richmond Hospital, &c.

THE subject of fractures engaging the extremities of the clavicle is one of some importance to the practical surgeon, inasmuch as these injuries are frequently the source of embarrassment in diagnosis. The absence of appreciable displacement in some of them, and the resemblance which others bear to dislocation, are the chief causes of this uncertainty. Having on a former occasion^b demonstrated by numerous dissections that the opinion expressed by several eminent writers, that fractures of the clavicle external to the coracoid process were unattended by displacement, was not sustained by facts, and that the existence of an extra-coracoid fracture was generally indicated by a well-marked deformity, I need not again allude to that subject. I shall, therefore, at present limit myself to a brief consideration of fractures engaging the sternal extremity of the bone.

The literature of the subject is extremely deficient, and opinions directly opposite have been expressed respecting the presence or absence of displacement. This is, no doubt, partly attributable to the comparative rarity of the occurrence of the injuries in question, but is also in some measure to be ascribed to a want of accuracy respecting the exact seat of the lesion of the bone. Authors have spoken in a general way of fractures of the sternal extremity of the clavicle, but have not with sufficient precision distinguished those which occur between the sterno-clavicular and rhomboid ligaments from those which are situated either between the fibres of or directly external to the latter structure; they have, in fact, omitted to mention the *exact* distance of the fracture from the sterno-clavicular articulation.

It seems to be generally believed that when the clavicle is broken internal to the rhomboid ligament, no displacement of either fragment occurs or can occur. The following is the statement of Ribes^c upon this point:—" Dans le cas où la solution de continuité

^a In 1859 and again in 1865, I drew the attention of the Pathological Society of Dublin to this subject.

^b Treatise on Fractures, &c., p. 209.

^c Mémoires de la Société Médicale d'Emulation. 1826.

arriverait à la portion de la clavicule placée au côté interne de l'attache du ligament costo-claviculaire, nul déplacement ne peut avoir lieu, ni par l'action musculaire, ni par le poids du membre. En effet, qu'il le fragment interne soit divisé en plusieurs pièces, ou qu'il soit entier, la capsule articulaire et le ligament rayonné antérieur, les muscles sterno-mastoïdien et pectoral, mettent ce fragment dans l'impossibilité de ce déplacer. Il ne peut survenir également aucun, ou presque aucun déplacement au fragment externe, puisqu'il se trouve retenu dans sa position par le ligament costo-claviculaire, par les muscles qui viennent d'être nommés, et par le sous-clavier."

"Si la fracture arrive un peu plus en dehors, malgré le poids du membre, le déplacement sera encore peu sensible, parce que le portion claviculaire du sterno-mastoïdien retiendra les deux fragments en haut; le grand pectoral les fixera en bas; et ils seront retenus postérieurement par le ligament costo-claviculaire, et par le muscle du même nom."

In the *Gazette des Hôpitaux* for 1845 the case of a patient under the care of M. Blandin has been recorded, where a fracture of the inner end of the clavicule is said to have occurred between the rhomboid ligament and the sterno-clavicular articulation, without displacement of either fragment resulting, the outer being maintained immovable by the first rib and the inner by the sternum.

In commenting upon this case Malgaigne, with his usual *brusquerie*, observes, "La théorie est fausse;" and in support of this assertion refers to two specimens preserved in Dupuytren's museum, in each of which a very considerable displacement exists. He further states that when the fracture exists, unaccompanied by external deformity, it is altogether due to the dove-tailing of the fragments and the integrity of the periosteum. I shall presently revert to these statements.

The opinions of Ribes and Blandin, quoted above, are in accordance with those held, I may say, by all who have alluded to the subject of fracture of the clavicule close to the sternal end of the bone, that is, between the costo and sterno-clavicular ligaments. I am inclined to doubt the correctness of these statements, which appear to me to be the result of theoretical reasoning alone, but I can make no positive assertion on the subject, being unacquainted with any recorded case in which the existence of fracture, either of recent occurrence or of ancient date, inside the rhomboid ligament has been demonstrated by dissection. A similar statement, as I have

already mentioned, was put forward respecting the extra-coracoid fracture, and believed to be true, until dissection detected its fallacy.

Those who maintain the doctrine of non-displacement in the intra-rhomboid fracture, seem to forget that the costo-clavicular ligament passes from its costal attachment upwards and *backwards*, and therefore is not likely to prevent the outer fragment of the broken clavicle from passing forwards to a certain extent. Any person can satisfy himself upon this point by simply sawing across the clavicle between the ligament and the sterno-clavicular articulation. He will find that he can then easily draw the outer fragment as far forwards as it is ever seen to pass in cases of fracture occurring close to the joint. The same result follows if the bone be divided through the centre or any other part of the ligament. Practically, as far as regards the question of displacement, it does not seem to be a matter of much consequence in what part of the bone comprised between the outer margin of the ligament and the joint, the fracture may be situated; the characters of the displacement are the same, the extent to which it is carried constituting, apparently, the sole difference.

I have already stated that the sources of information at my disposal have not furnished me with a single instance, proved by dissection, of the occurrence of an intra-rhomboid fracture; but cases have been recorded (and I have myself seen such) in which the seat of fracture, indicated by the projection of the outer fragment, has been ascertained by measurement to be situated only half an inch from the sterno-clavicular articulation. It may not, perhaps, be anatomically accurate to designate these as true intra-rhomboid fractures; but they may be considered as such as far as regards the occurrence of displacement.

A question here suggests itself: if the rhomboid ligament is adequate to prevent the displacement of the outer fragment, when the bone is broken internal to it (that is, as close as possible to the joint) why does it seem to have so little influence in counteracting the displacement in cases of luxation forwards of the sternal end of the bone? The rupture of the sterno-clavicular ligaments seems hardly of itself sufficient to account for it; and I think we must *partly* ascribe it to the circumstance, to which I have already alluded, namely, that the rhomboid ligament, though normally directed backwards from the first rib and its cartilage, permits of that direction being altered to one forwards, when the inner part of the clavicle passes forwards, and therefore only opposes *within*

certain limits, the displacement of the sternal end of the bone in cases of luxation, or of the outer fragment in cases of fracture.

In considering the question of the occurrence or non-occurrence of displacement in cases of intra-rhomboid fracture, it is obviously of importance to have accurate ideas respecting the anatomy of the rhomboid ligament. According to Struthers, who has described the clavicle and its connexions with a greater degree of minuteness than, perhaps, any other author, the attachment of this ligament is from an inch to an inch and a quarter in length, beginning very close to the articular margin. It passes obliquely upwards and backwards, and thus runs obliquely upon the bones, resembling in this respect the trapezoid ligament; it reaches back to the lower boundary of the posterior or cervical surface of the bone.

With respect to the sternal extremity or head of the clavicle, its posterior or prolonged angle gives attachment to two strong ligamentous bands, one arising from the lower part of the posterior edge of the sternal notch, passes upwards and outwards, while the other, running from the costal cartilage, passes upwards and inwards, appearing as though it were the inner end of the rhomboid ligament; but, according to the above-named writer, it is properly a more posterior and internal band. Those two strong ligaments, each about one-third of an inch broad, meet by their neighbouring edges, and embrace the posterior angle.

It will be seen from this description how exceedingly small is the space between the costo-clavicular and sterno-clavicular ligaments—so small, indeed, that as far as regards the phenomena of displacement, these ligaments may be looked upon as constituting one continuous structure, extending as far as the external border of the former, that is, an inch and a quarter from the articulation; and it may, I think, be reasonably supposed that in whatever part of this extent a fracture may be situated, the general characters of the displacement will be similar.

The fact of the sternal portion being firmly joined to parts which are fixed, while the outer or acromial portion is connected with parts which enjoy an extensive range of motion, affords a ready explanation of this circumstance. The principal difference as regards the displacement of the fragments between fractures situated at the sternal end of the bone and those engaging the shaft, seems to be that in the former the inner end of the outer fragment is directed

forwards, but in the latter it passes backwards, a difference which it would not be very difficult to account for.

In the *Medical Press* of February 9, 1870, Mr. Croly has recorded the case of a female, aged 76, in whom the sternal end of the left clavicle was broken half an inch external to the sternoclavicular articulation. The extremity of the outer fragment formed a remarkable projection in front, similar to that seen in cases of luxation forwards of the sternal end of the bone. He also mentions the case of another patient sent into hospital with a fracture similarly placed, but which was supposed to be a dislocation of the inner end of the clavicle forwards.

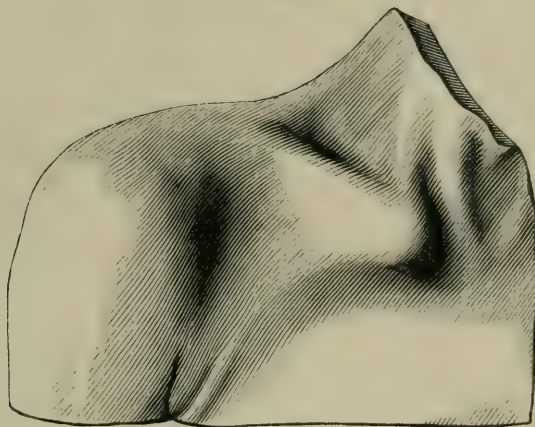
In the article upon injuries to the upper extremity in *Holmes's Surgery* allusion is made to two cases which occurred at the Middlesex Hospital, but the nature of the displacement is not mentioned. Both patients were under ten years of age. In one the fracture was about half an inch from the sternoclavicular joint, and the displacement was very slight; in the other the lesion of the bone was a little further from the articulation, and there was more displacement.

In the case published by Lonsdale, which so many subsequent writers have alluded to, the patient was only three years of age; the author has stated that there was no displacement, and that the fracture was situated half an inch from the sternal joint, and he believed that the epiphysis was detached. The anatomist will at once perceive that this idea is directly opposed to the statement respecting the seat of fracture.

In the article "Fractures," published in the *Cyclopædia of Practical Surgery*, Mr. King alludes to three specimens preserved in the museum of Guy's Hospital, in each of which the clavicle has been broken about an inch from its sternal extremity, but the only description of the displacement which he has given, is to the effect that in one of them "the inner broken surface presented upwards." In my opinion this is an inaccurate statement, inasmuch as it implies the displacement of the sternal fragment, an extremely improbable occurrence. It *seemed* to present upwards, I imagine, because the outer fragment was, as it usually is, displaced downwards.

The following five cases of the injury under consideration came under my own immediate notice, and in each of them the situation of the fracture was carefully ascertained:—

Fig. 1.



FRACTURE.

Fig. 2.



DISLOCATION.

Fig. 1.

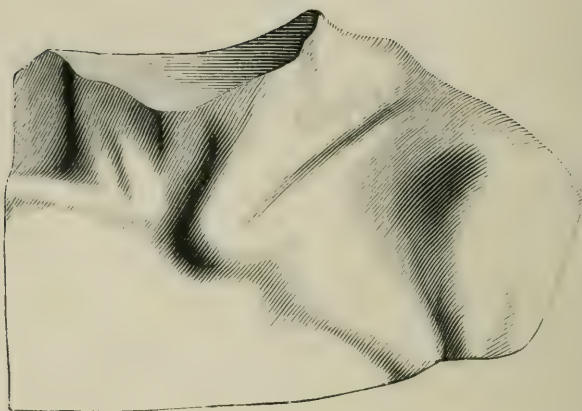
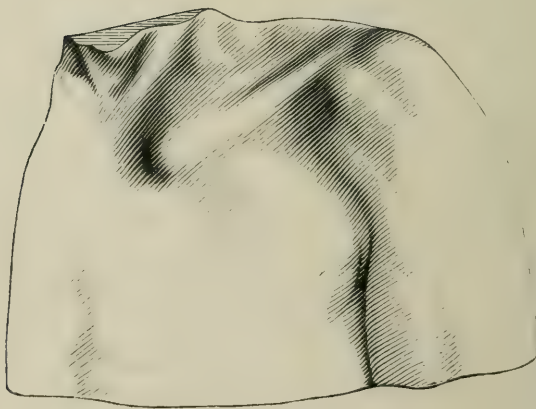


Fig. 2.



FRACTURES.

CASE I.—*Fracture of the right Clavicle, situated less than three-quarters of an inch outside the sternal Origin of the sterno-mastoid Muscle.*—The fracture was transverse, and the inner extremity of the external fragment formed a very abrupt and striking projection, but did not overlap the inner fragment, neither was it drawn downwards to any appreciable extent. It had carried forwards the clavicular portion of the sterno-mastoid, causing it to stand out in relief. The deformity bore a very striking resemblance to that which accompanies luxation of the inner end of the clavicle forwards, but the diminished length of the bone sufficiently marked the true nature of the injury. (Plate I., Figs. 1 and 2.)

CASE II.—*Fracture of the left Clavicle, three-quarters of an inch from the sterno-clavicular Joint, nearly corresponding to the inner border of clavicular portion of the sterno-mastoid Muscle.*—The outer fragment had suffered a very considerable amount of displacement, and the resulting deformity was great in proportion. Its inner extremity had suffered a three-fold derangement: it was strongly drawn forwards, and displaced so far downwards that it lay nearly an inch below the level of the sterno-clavicular articulation of the opposite side. It had also passed inwards to such an extent as to overlap slightly the margin of the sternum. The clavicular portions of the sterno-mastoid and great pectoral muscles stood out in bold and striking relief. In this case the humerus and several of the ribs were also broken. (Plate II., Fig. 1.)

CASE III.—*Fracture of the left Clavicle, one inch and a half external to its articulation with the Sternum, attended with great Deformity.*—In this instance, as in the preceding, the outer fragment had undergone a three-fold displacement, namely, downwards, forwards, and inwards. Its inner extremity was more than an inch below the level of the sterno-clavicular joint. The clavicular portion of the sterno-mastoid was in relief, and the outer end of the sternal fragment seemed to be slightly elevated, but of this I am not by any means certain. (Plate II., Fig. 2.)

CASE IV.—*Fracture of the left Clavicle, situated at a distance of nearly two inches from the sterno-clavicular Articulation.*—The injury had occurred many years before the cast was taken. The inner extremity of the outer fragment formed an abrupt and striking projection in front of the sternal fragment, but was not displaced in

any other direction. The fracture seemed to have been directly transverse. The functions of the shoulder and arm had been perfectly regained.

CASE V. —*Fracture of the left Clavicle, one inch and a quarter external to its articulation with the Sternum.*—The acromial or outer fragment has been displaced forwards, carrying with it a portion of the clavicular origin of the sterno-mastoid muscle. The sternal fragment remained *in situ*.

After alluding to the displacement forwards and downwards of the outer fragment in the two specimens preserved in the museum of Dupuytren, Malgaigne asks the question:—"Ce mode de déplacement serait-il constant? Je pose la question sans la résoudre, ne connaissant pas d'autres exemples de cette fracture que ceux que je viens de rapporter." So also Anger:—"Ce mode de déplacement présente-t-il un type ordinaire dans ces fractures très-rares, et le plus souvent de cause directe? C'est une question à laquelle je ne saurais répondre."

I think it may be safely said that we are now in a position to reply to this question in the affirmative. In ten cases which I have either described or referred to in this memoir, the direction of the displacement was the same, the inner end of the acromial fragment projecting in front, and in most of them having been also drawn downwards and inwards. In none of them has the inner or sternal fragment appeared to have undergone any derangement of position; and I may here remark that whatever may be the nature and amount of the influence of the ligamentary apparatus connected with the sternal end of the clavicle, in preventing displacement in fractures of this portion of the bone, it is upon the internal fragment that it is principally exerted. This usually remains *in situ*, while the acromial fragment suffers displacement, and this seems to be the case irrespective of the distance of the seat of fracture from the sterno-clavicular articulation.

In some instances the outer fragment is simply displaced forwards, the projection being on the same level as the sterno-clavicular articulation of the opposite side. In such cases the fracture will usually be found close to the joint, and the resemblance to luxation greatest. But when the three-fold displacement—viz., forwards, inwards, and downwards—is present, I think we may look upon it a certain indication that the lesion of the bone is altogether external

Fig. 1.

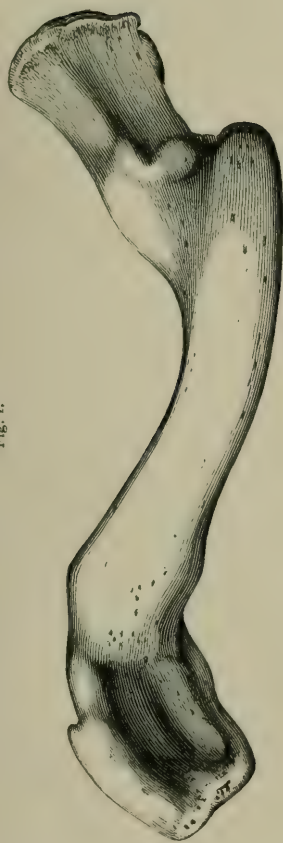
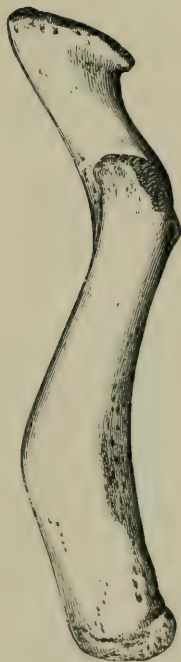


Fig. 2.



FRACTURES.

to the costo-clavicular ligament, for the instances must be very rare in which this powerful structure is ruptured by the force which broke the bone. Indeed, I have generally found that the amount of displacement downwards was a tolerably accurate measure of the distance of the fracture from the sterno-clavicular articulation, this displacement being less in proportion as the lesion of the bone is nearer to the joint.

In the allusions that have been made to the intra-rhomboid fracture a want of accuracy, as I have already stated, has prevailed respecting the exact distance of the lesion of the bone from the sterno-clavicular articulation, that has deprived them of much of the value they would otherwise have possessed. Perhaps I might go so far as to say that no evidence has yet been adduced of the actual occurrence at all of this fracture. There is certainly none derived from dissection, and but little that is satisfactory from the descriptions and statements of writers—statements some of which would almost lead us to suppose that their authors were not acquainted with the exact position and extent of the rhomboid ligament. I take up the elaborate treatise on fractures by Hamilton, by far the most distinguished among the American writers on the subject, and what do I find? At page 182 of the first edition there is a brief detail of the case of a lady, aged eighty years, who fell down a flight of stairs, and broke the right clavicle *about one inch from the sternum*. Crepitus was distinct, but there was scarcely any displacement. That the author considers this to be an instance of *intra-rhomboid* fracture is evident from the following passage at page 183:—"If the fracture is near the sternum and *within the fibres of the costo-clavicular ligaments, as in the case of the old lady just cited*, the displacement is inconsiderable."

I have already alluded to the assertion of Malgaigne that the theory of Blandin, and therefore that of Ribes, was false, and have mentioned that in support of this opinion he has relied upon the evidence of two specimens preserved in the "Musée Dupuytren." One of these he has delineated in the atlas accompanying his great work on fractures and luxations.* It will be remembered that Blandin's statement refers solely to fractures inside the costo-clavicular ligament; we are, therefore, justified in presuming that in each of the specimens to which Malgaigne has appealed, the fracture was likewise situated within that ligament. The fact,

* Plate III., Fig. 8.

however, is far otherwise. In the printed catalogue of the museum^a (which I happen to possess) the specimens in question are marked Nos. 64 and 65. The fracture is similarly situated in each, and in the description given of No. 65 (that delineated by Malgaigne) it is distinctly stated that the fracture is "*en dehors de l'insertion du ligament costo-claviculaire.*" Indeed, a glance at the accompanying engraving of this preparation (Plate III., Fig. 1) is sufficient to show that the fracture was not internal to the ligament, for the broken extremity of the inner fragment is fully an inch and a-half from the sterno-clavicular articulation. I should be sorry to ascribe the statement of Malgaigne to intentional misrepresentation, and would rather attribute it to his ignorance of the true anatomy of the rhomboid ligament; and yet, on the other hand, it is difficult to suppose that he was unacquainted with the description of the specimen given in the printed catalogue of the museum.

Anger has represented a fracture of the clavicle similarly placed, and attended by exactly the same deformity.^b I have reproduced this engraving in Plate III., Fig. 2; the extremity of the sternal fragment is distant one inch and a quarter from the joint.

In order to render the history of the displacement in fractures of the extremities of the clavicle more connected, I venture to reprint upon the present occasion some of the conclusions at which I formerly arrived^c respecting fractures of the acromial extremity of the bone, conclusions which subsequent experience has not induced me to alter.

1. In fractures of the acromial extremity of the clavicle, between the coraco-clavicular ligaments, when displacement occurs, it is in general limited to an alteration in the direction of the bone, by which the natural convexity of this portion of the clavicle is increased.

2. In cases of fracture between the trapezoid ligament and the sterno-clavicular articulation, the displacement of the outer fragment is considerable, and is frequently carried to such an extent that the fragments form a right angle with each other.

3. The doctrine of non-displacement in intra-rhomboid fractures has not yet been established upon so secure a basis as to justify its acceptance as a truth.

4. In intra-rhomboid fractures the displacement of the sternal

^a Musée Dupuytren, publié au nom de la Faculté. 1842. No. 65, p. 85.

^b Traité Iconographique des Maladies Chirurgicales. 1865. Page 124.

^c Treatise on Fractures, &c., page 222.

fragment, if it ever occurs, is the exception; that of the acromial fragment, to a greater or less extent, constituting the rule.

5. The explanation is, that the former is firmly connected by strong ligaments to fixed and immovable parts, while the latter is under the influence of three causes of derangement of position, viz., muscular action, the weight of the upper extremity, and the mobility of the scapula.

6. The influence of these causes in producing displacement of the outer fragment is not confined to intra-rhomboid fractures.

7. In fractures of the inner extremity of the clavicle the nature of the derangement of the acromial fragment is uniform, and its direction constant, viz., forwards. The first rib offers a mechanical obstacle to its displacement backwards.

8. The rhomboid ligament, from its direction, would permit of the outer fragment being displaced forwards and downwards, to a limited extent, were the bone broken internal to it.

ART. III.—*Senile Contraction of the Vagina.* By ALFRED H. M'CLINTOCK, M.D., F.R.C.S., &c.; ex-Master of the Dublin Lying-in Hospital; Hon. President of the Dublin Obstetrical Society; Hon. Fellow of the London Obstetrical Society; Corresponding Member of the Edinburgh Medico-Chirurgical Society; Hon. Member of the Gynæcological Society of Boston (U. S.); &c., &c.^a

OF the lesions to which the vaginal canal is subject, we find that contractions, contorsions, or occlusion by cicatrices or adhesions, form no inconsiderable proportion. These result in most instances from inflammation and sloughing consequent upon parturition. Dr. Gaillard Thomas, in his *Practical Treatise on Diseases of Women* (Philadelphia, 1868), gives the following enumeration of the causes which may produce this condition of the genital canal, viz.:—"Arrest of development; prolonged and difficult labour; chemical agents locally applied; mechanical agencies; sloughing, the result of impaired vitality; syphilitic or other extensive ulcerations."

Atresia of the vagina, arising from inflammation and sloughing, is familiar to all practical obstetricians. Narrowing, shortening, or

^a Read at Obstetrical Society, 11th June, 1870.

occlusion, as a congenital malformation, is also well known and fully described in various treatises upon female complaints. But the condition of the vagina which forms the subject of the present communication, differs apparently from both these groups, though in common with them presenting as its leading feature a morbid contraction of the canal at some one part. It is not, however, a *congenital*, but an acquired defect; neither is it a sequel of parturition, nor, so far as my observation goes; of any inflammatory or adhesive process.

Although some years have elapsed since I first recognized this state of the vagina as a distinct lesion, I can give but a very imperfect account of it. I know nothing of its etiology, nor have I had any opportunity of making an anatomical examination of the parts affected, so that I am equally ignorant of its pathology.

In what I may call the minor forms of this contraction, we find simply a transverse fold of the vagina projecting inwards, and presenting to the finger a sharp crescentic edge. This seldom engages more than the one-third or one-half the circumference of the canal, and is generally situated on the posterior wall, a little better than half-way up; but I have also met with it higher, or implicating the os itself by one or other extremity of the semilunar, valve-like, projection. These partial contractions have been noticed by Dr. Byford in his work on *The Practice of Medicine and Surgery applied to the Diseases and Accidents incident to Women* (2nd Ed., Chicago, 1867). I shall take the liberty of quoting the passage, as it contains the only allusion I can find in systematic treatises upon gynecology, to the particular lesion we are now considering:—"There is often a cord or frenum-like projection in the vaginal walls, which is planted into the external surface of the anterior and posterior lips of the mouth of the uterus, and thus extends backward and forward to be lost in the anterior and posterior median line of the walls of the vagina. This frenum is more apparent, if not more developed, as women advance in age; but I have known it so prominent as to be mistaken for the results of disease even in the middle-aged. In one case an intelligent practitioner thought it an evidence of the injurious effect of strong caustics."

There is another and more important variety of this species of contraction, which I have occasionally observed, and to which I now beg to direct your attention.

It is hardly necessary for me to remind you that the upper part

of the vagina, in the normal and healthy state, is very capacious and distensible. The finger can be freely passed in front, behind and around the cervix uteri, to the full depth of the vaginal cul de sac. In married women, and especially those who have borne children, this capaciousness is more remarkable than in virgins, but even in these the finger can be freely carried around the cervix. This state of things is quite altered, however, by the species of contraction I now wish to describe, in which there takes place a progressive diminution of the calibre of the vagina—not throughout its entire extent, but commencing at its summit, and slowly advancing downwards. When the contraction has reached the level of the os tincœ, the introduction of the finger into the vaginal cul de sac around the cervix becomes quite impossible, this part (cervix) being so closely embraced by the broad ribbon-like stricture. With the persistent increase of the constriction the os and cervix become quite encapsulated, and beyond the reach of touch or sight. The foramen through the stricture, in two of my cases, was so small as barely to admit a probe, and might very readily have been mistaken for the os uteri itself. How much lower down this process of contraction may extend, I cannot at present say, the cases which have been longest under my observation being married women, and I should imagine that sexual intercourse would tend to hinder or retard the progress of the contraction downwards.

The feel of the constricted part of the vagina did not present anything peculiar beyond the simple narrowing of its calibre. There was no perceptible induration to the touch, or change in the appearance as seen through the speculum. The feel and condition of the part, therefore, presented characters very different from what we find in the cases of vaginal constrictions resulting from cicatrices supervening on inflammation and sloughing. In none of the cases was there the remotest ground for supposing that the contraction of the vagina could have been so produced. Neither did it in any instance depend on cancerous or other organic disease; but yet, I do not see anything impossible in this complication taking place.

All the cases I have met with were women advanced in years—between 50 and 60—only one was under 50; and on this account I am disposed to regard it as some sort of atrophy connected with the decline of life, and the extinction of functional activity in the generative organs. On this presumption, as well as from the want

of a better designation, I have ventured on calling it "Senile contraction of the vagina," which may serve to distinguish the lesion till some better name be found, or till its dependence on some other lesion as a cause be established. In old women I have often seen considerable narrowing of the uterine orifice, with a contracted or impervious condition of the cervical canal; and this I would refer to the operation of a like cause as that which induces the vaginal contractions I have been speaking of. Again, the shrivelled, atrophied state of the uterus so common in women far advanced in years, may probably be regarded as another illustration of the same process.

The *diagnosis* of these senile contractions of the vagina was easily made in every instance, and I can hardly imagine a difficulty presenting itself to any careful examiner possessing an ordinary amount of tactile experience. Still, it may be useful to mention that I have known this stricture of the vagina mistaken for the contraction of scirrhus deposition; and Dr. Byford, as already stated, mentions a case where an intelligent practitioner thought the stricture was the effect of strong caustics!

All the examples that have fallen under my notice of this lesion were married women. Some had been sterile, but more than half had borne children. Only one patient was a widow: all the others were married, and living with their husbands. In every instance the menstrual function had ceased, even in one of the cases where the lady's age did not much exceed forty years.

The evidence I at present possess leads me to look upon this affection as a comparatively unimportant one, so far as regards prognosis and treatment, inasmuch as it does not seem to have any injurious influence upon the local or general health. The patients in whom I detected the lesion are all still living, I believe, though it is several years since I ascertained its existence in some of them. The chief interest, therefore, of a practical character, which attaches to it has reference to the great question of differential diagnosis, from the liability of its being mistaken for serious organic disease of the vagina or uterus.

Although scarcely noticed in books, I have little doubt that most practitioners of large experience in female complaints, have met with examples of this *senile contraction of the vagina*. Dr. Matthews Duncan, of Edinburgh, states, in reply to an inquiry on the point, that he has seen many such.

Before concluding, I would wish to offer a word of apology for

the incompleteness of the present communication, and to express the hope that other observers would investigate this lesion, and use their opportunities to make out its true pathological nature.

ART. IV.—*On Certain Forms of Paralysis and their Treatment by Electricity.* No. II. By WALTER G. SMITH, M.B.; Fellow and Censor of the College of Physicians; Assistant Physician to the Adelaide Hospital.

IN the spring of last year I read before the Medical Society of the College of Physicians a paper on the electrical treatment of certain forms of paralysis. The cases then detailed fell under the four heads of Facial Palsy, Lead Paralysis, Local Traumatic Paralysis, and Rheumatic Paralysis.

None of these groups will come under review on this occasion, with the exception of traumatic paralysis, a few additional examples of which will be mentioned; and I intend now to refer especially to two very different and serious affections, viz., Paralysis of the Bladder, and Organic Infantile Paralysis.

As in the former examples, so, also, in the cases of traumatic paralysis to be presently alluded to, the paralysis was due to compression or injury of the nerve interfering with its physiological conducting power, and not to any solution of its anatomical continuity.

I. TRAUMATIC PARALYSIS.

CASE I.—In January, 1870, J. Walshe, a tailor, aged 26, applied at the Dispensary, on account of loss of power in the left hand. On December 27th he fell asleep while drunk, and when he awoke he found that the left hand and arm were numb and powerless. He usually sleeps on the left side.

Notwithstanding the use of a stimulating liniment for some days, the arm has remained in much the same condition. There is complete drop-wrist, also partial anæsthesia to touch on the back of the hand and along the first three fingers, and increased sensibility to cold in the paralysed parts.

Faradization was resorted to twice or thrice a week, and in a month he was able to resume work, and soon perfectly regained the use of the arm. The electric excitability was normal, and the anæsthesia was removed after the third sitting.^a

^a It may be well to state at the outset that when the term Faradization (*i. e.*, the local application of *induced* currents) is made use of, no demarcation is drawn

CASE II.—Mr. W. F., aged 36, was recommended to me by Mr. Clendinnen in May, 1869, and gave the following account:—About eleven days previously he fell asleep, while under the influence of drink, with his head resting on the right arm. On awakening the arm felt “dead and numb,” and since that time the limb is easily chilled, and though stimulating liniments were resorted to the paralysis showed no sign of amendment. The loss of motility only involved the extensors of the wrist and fingers; the power of supination was retained. Induced electricity was at once resorted to, and in less than a month, after but ten sittings, a nearly perfect cure was effected. He was then obliged to leave Dublin, but I heard from him some time after, to the effect that his arm was almost well, and that he had derived great benefit from the continued use of a volta-electric machine.

It is remarkable that during the first week of treatment the current was much more acutely felt, and produced far stronger contractions on the paralysed than on the non-paralysed limb. This is not usual in traumatic paralysis. It was observable, too, that the paralysed muscles did not respond so actively at the first instant of stimulation as in a few minutes after—it seemed as if the contractility of the muscles required a little time to fully manifest itself.

CASE III.—Jane Edwards, aged 56, came to the Dispensary on May 26th, 1869, complaining of weakness in the left fore-arm. Three nights previously she was leaning out of a window on that arm for about half an hour, the right elbow resting in the left palm. At the end of that time she found that she could not extend the hand, and felt tingling and numbness in the thumb, index and middle fingers. Some benefit was perhaps derived from the use of a liniment; but with the employment of Faradization the arm began to mend at once, and in a fortnight she had quite recovered the use of the muscles. During the whole time she was

between the so-called primary and secondary current. Duchenne, in particular, has sought to establish a distinction between them as to their respective actions on muscles and nerves. I have never been able to satisfy myself that there is any real difference, and believe that, if we simply keep in mind the greater intensity of the secondary current, and its consequent higher penetrating power, it is practically immaterial whether a stronger primary or a weaker secondary current is employed. This is but natural, considering the similarity in the mode of production of the two currents. Dr. Russell Reynolds has, I am glad to find, arrived at the same opinion. (*Lancet*, March 5th, 1870.)

under observation the same facts were noticed as in the last case, viz., exaltation of the electric sensibility in the paralysed arm, and increased response to the electric stimulus.

Recovery would, no doubt, have been even much more rapid in these cases if daily Faradization had been possible.

CASE IV.—In civil practice we seldom have an opportunity of seeing the effects of gunshot wounds of nerves, but the case I am about to mention is one of a class of injuries now unhappily too common.

Last November my friend Mr. S. Knaggs sent Mr. W. P., a farmer, to me for electrical treatment. Mr. P., while driving in his gig on the 5th of January, 1869, was fired at in consequence of an agrarian dispute, in which he had become involved. He was struck in the left arm and the left side of the chest, and as the gun was discharged close to him, he would probably have been mortally wounded were it not for the protection which three coats afforded. A considerable amount of blood was lost at the time. The marks of twelve cicatrices of different sizes were visible on the inner and under surface of the left arm, above the elbow-joint, some of them very small, being caused by grains of shot, others larger and more irregular caused by slugs. One grain of shot remained imbedded under the skin, close to the median basilic vein, having travelled from behind forwards, for there was no trace of wound anteriorly. Common sensibility is impaired along the little finger, and ulnar side of ring finger, and he cannot distinguish between a wet and a dry object, nor take cognizance of the nature of any substance he touches. He is acutely sensitive to tickling, and to the sudden impression of a point, which produces a feel like a foot awaking out of "sleep."

Left fore-arm measured 25·5 centim. (10 in.), right 26·5. (10·5 in.). He is a left-handed man. A heated body is not felt so plainly along the track of the left ulnar as along the right ulnar nerve. The muscles of the left wrist are weakened, the ball of the thumb is flattened and flabby, and the fork of the first finger is atrophied—and, indeed, all the interossei are enfeebled. Faradization was as soon as possible adopted. The grain of shot, which lay under the skin close to, and probably touching, the internal cutaneous nerve, was removed. The extraction of the grain of shot gave him great relief, and he was now able to rest his arm on the table with comfort, a position which previously caused pain. The

extensor muscles and interossei responded freely and perfectly to electric stimulation, an omen of speedy recovery; but a moderate current which was acutely felt on the sound arm was not perceived on the left arm at its lower half. In a few days, under the use of Faradization, he felt as "if the life had come back into the arm;" the feeling of cold and the numbness along the ulna disappeared, and in a short time he was perfectly well. The noticeable features in this history are the long-continued impairment of sensibility and loss of power due to the disturbing effect of a small local injury to the nerves, and the rapid cure following the means adopted.

A few other points suggested by these cases and those already published, require a word or two of comment.

It has been mentioned incidentally once or twice, that stimulating liniments, though so generally prescribed, have proved of little or no service. The experience of others will, perhaps, confirm this opinion; if so, the less time that is wasted before the direct stimulation of nerve and muscle is resorted to the better. The liniment, if so desired, can be used from the first along with the electric treatment. The fact of the electric sensibility and excitability being heightened in two cases may be recalled, and it is not easy to offer any satisfactory explanation of this phenomenon. A very remarkable circumstance, and one of physiological interest, is exemplified in many, if not most, cases of traumatic paralysis, and is illustrated in the foregoing instances. It is the want of correspondence between loss of motion and loss of sensation. Anæsthesia seems to be a less frequent sign of partial nerve lesions than loss of motility, and it is certainly less permanent and more amenable to treatment. Now, why should this be so? When a compound nerve is injured, it is very difficult to explain how it so often happens that total motor paralysis is caused, and little or perhaps no sensory paralysis. We cannot, by the most refined chemical or histological analysis, distinguish between a sensory and a motor nerve fibre, and all our present knowledge points to the intimate and inseparable commingling of the two classes of fibres in the same trunk. Yet, an injury to a compound nerve does not, as a rule, equally damage its motor and sensory fibres, for, "practically, it is the motor filaments which suffer most severely, most often, and most extensively."^a No plausible explanation of this has

^a On this, as well as on many other questions of nerve pathology, consult the admirable little work of Drs. W. Mitchell, G. Morehouse, and W. Keen, on *Gunshot Wounds and other Injuries of Nerves*. Philadelphia: Lippincott & Co. 1864.

yet been offered, though Dr. Mitchell and his colleagues (*op. cit.*) throw out an ingenious hypothesis, which attempts, in part, at least, to explain the anomaly. Is it possible that the *sensitive* fibres of adjoining nerves so inter-communicate that, when a nerve is injured or even cut across, the function of sensation may be carried on by the anastomoses between it and the adjacent filaments? The existence of sensibility in the distribution of a nerve some considerable time—thirty months—after as much as two inches of it (the median nerve) had been excised, lends some probability to such a view.

In relation to prognosis, it should be remembered, and this is especially insisted upon by the authors already quoted, that the effects of concussion of a nerve may be as serious as those of direct injury or section of the nerve, and that the remote consequences of a nerve-contusion may be as mischievous to the nutrition of a limb, and as permanent, as those which follow total destruction of the nerve.

II. PARALYSIS OF THE BLADDER.

Paralysis of the bladder is known sometimes to follow on over-distension of that viscus, especially in old people, and is now and then observed as a complication of continued fever. We are also familiar with it, in one of its most hopeless aspects, in conjunction with paralysis of the lower extremities, dependent on disease of the spinal cord.

Paralysis of the bladder, *per se*, is not so commonly met with unconnected with loss of motor or sensory power in other organs, or with centric lesion, but the case I will immediately advert to is an example, and an extreme one, of complete vesical paralysis, attended with perfect anæsthesia of a sharply defined region, and unassociated with any other motor or sensory derangement.

M. P., aged 33, was taken into the Adelaide Hospital by Dr. Richardson, in January, 1869. He is a jeweller by trade, and is sometimes obliged to sit working for fifteen hours in the day. He has had gonorrhea three or four times; the last attack dated nearly a year and a half back. About six months ago he perceived that the perineum and inside of the thighs were numb, and that he had not complete control over the bladder. These symptoms were preceded, for some time, by pains running through the limbs. For the last four months he has been compelled to use a catheter almost every day, and urine often dribbles away as he walks, or

when he coughs. The prostate is not enlarged; there is no irritability of the bladder, and he states that the urine always appeared of a natural character. There is not the least evidence of any paralysis, direct or reflex, of the lower extremities. He has not had intercourse for sixteen months, and his virile power is diminished. Sometimes control over the rectum is lost, and the stools pass involuntarily.

The whole of the perineum, as far back as the tubera ischii, and lower part of sacrum—the scrotum and penis—even the glans—are almost completely anæsthetic to the strongest induced current applied by the wire brush, the most powerful means of testing sensibility. The scrotum can be tightly pinched without causing pain, and he is quite indifferent to the hairs being roughly pulled out. A heated spatula is tolerated without inconvenience on these parts, so hot as to cause instant pain when touched to the thighs. The line of analgesia is strictly symmetrical and abruptly limited, and extends up to the external abdominal rings. Within a space of half an inch the transition from the normal sensibility of the adjacent skin to the insensitive tract, was sudden and unmistakable. Tactile sensibility is preserved.

The effects of treatment may be briefly summed up.

After five applications of the current there was decided increase of the sensibility, and Dr. Richardson found that whereas at first a No. 13 catheter easily slipped into the bladder, a No. 9 was now grasped by the urethra. In a few days he began to regain power over the bladder, and re-acquired command of the rectum, and by the end of March could feel the passage of the catheter. For some months I lost sight of him, and in September he was in much the same way, but could not completely control the functions of the rectum—flatus, and even some fæces, escaping involuntarily.

He was ordered tincture of ergot and perchloride of iron. The Faradization was resumed, and by the middle of October the upper half of the scrotum had partially regained its sensibility. He was next put on a combination of nux vomica and ergot, and on December 17th was much better; retained the urine more completely, and felt a strong current slightly on the penis, and even on the perineum. The electricity was applied as often as he presented himself, and in the beginning of March he had almost perfect command over the bladder, except that he was obliged to strain a little during micturition.

The mode in which the current was employed was as follows:—

The induced current was transmitted into the bladder by means of a bougie electrode, made for me by my friend Mr. C. Ball, connected with the *negative* pole; and a moistened sponge attached to the positive pole was pressed firmly on the hypogastrium, and occasionally placed at the side of the sacrum. Very little benefit would, I believe, have followed from applying one moist conductor above the symphysis pubis, and the other on the lower part of the spine, as is sometimes recommended. The current was seldom passed through the bladder for longer than ten minutes at a time, and in electrifying this organ the possibility of provoking catarrh of the bladder by too strong a current should not be disregarded.

For the relief of the analgesia the wire-brush was constantly resorted to; and to the prolonged use of this, and of the conducting bougie, the success of the treatment is to be attributed.

The bladder seems to be but slightly sensitive to the passage of even an intense current. In the case of the bladder or other non-striated muscle, it is not necessary that every part of the paralysed muscle should be locally stimulated, for it is one of the peculiarities of smooth muscular fibres, in contradistinction to striped muscular fibre, that the contractions excited in them persist for some time after the cessation of the irritation, and, moreover, spread gradually from the muscular fibres in which they begin to the adjacent fibres. Besides, in the case of smooth fibre, the direct stimulation of the muscle exercises a more powerful influence than mediate excitation through the nerves. The contrary holds good with striated muscles.*

The persistence of the anæsthesia after the paralysis was cured is a noticeable feature, for, as a general rule, the prognosis in anæsthesia is much more favourable, even when resulting from organic disease of the nervous centres, than in motor derangements.

The patient's virile power was also materially improved during the continuance of the treatment.

When it is remembered that in this case the incontinence of urine had lasted six months before he was placed under electric treatment, that he had been for a considerable time under medical attendance, was gradually sinking into a more hopeless condition, and was transferred to me as a forlorn hope, the result of the treatment is, I think, eminently satisfactory. It is true, the Faradization

* See MM. Onimus and Legros. De la Contraction des Muscles de la vie Vegetative. Journ. de l'Anat. et de la Phys. No. 4. 1869.

was prolonged over many months; but in such a case this is not discouraging; and of this there can be no doubt, that the first signs of improvement dated from the time that the electricity was systematically directed to the affected organ.

III. ORGANIC INFANTILE PARALYSIS.

Passing now to the subject of infantile paralysis, I wish particularly to urge the importance of early and persevering treatment in this grave affection, and to indicate the grounds on which a less gloomy view of its prognosis than is often taken may be rationally adopted. Before doing so it should be clearly understood, that by the term "Organic Infantile Paralysis" (Hammond) is meant only that class of cases which occur within the earliest years of life, and which are characterized not only by paralysis of motion, but also by extreme atrophy of the muscles and stunting of the bones, and by a cold and cyanotic state of the affected parts. The malady is often known by the name of *essential paralysis*, and has also been described under the titles of *fatty atrophic paralysis of childhood*, *paralysis during dentition*, &c.

This obscure and not unfrequent disease, though its pathology is not thoroughly made out, and we are ignorant of the precise nature of the morbid process, appears, in all probability, to be a spinal affection, and requires to be carefully distinguished from those forms of paralysis occurring in children as a consequence of cerebral mischief. The advanced atrophy, the depreciation of temperature, and especially the diminution or total abolition of electro-contractility sufficiently discriminate the spinal from the cerebral paralysis.

It is scarcely necessary to remark that it differs in several essential particulars from progressive muscular atrophy, and though, like the latter, the muscles are unequally affected, yet the peculiar dissecting character of Cruveilhier's atrophy is wanting in infantile paralysis.

The muscles, in a large number of cases, undergo fatty degeneration; but Drs. Hammond and Volkmann have shown that this is not invariably the case, even in the most severe instances of the disease, and after the lapse of years. The degree of paralysis does not, therefore, strictly correspond, as Duchenne maintained, with that of fatty change. The bladder and rectum are not implicated. Besides the depression of the nutrition of the paralysed limb, we have to dread, as secondary consequences, changes in the shape and

direction of the articular surfaces, and paralytic contractions, giving rise to clubbed feet and other deformities.

CASE I.—J. Harrison, aged two, with dark eyes and blue sclerotics, was brought to the dispensary in the close of April, 1869, by his mother, for advice as to wasting and loss of power in the left leg below the knee. He has always been a delicate child, and liable to attacks of diarrhœa, and has passed through measles and whooping cough. A year ago he had convulsions while teething, and is at present troubled with ascarides. About three or four months since his mother noticed that the left leg was smaller than its fellow, and from that date up to the present the atrophy has been gradually advancing. When he attempts to stand, which he cannot manage to do without support, he inclines to fall to one side, and the left foot turns down on the outside, and is dragged sideways along the ground. When the leg droops the foot is turned in, and the ankle joint is so mobile that the foot can be shaken about as if it hung from a hinge. The left thigh is one quarter of an inch (0·6 cm.) smaller than the right, and the left calf three-quarters of an inch, (1·8 cm.) and the child often complains of that limb being cold. The paralysed muscles are soft and flabby, and the whole nutrition of the limb is seriously impaired. Tickling the left sole excites no reflex contractions, while these are easily provoked on the right side. On the application of an induced current, electro-contraction was greatly diminished. A battery current from 10 Daniell's cells produced no decided effect. For a month I lost sight of him, and on re-examination not the slightest contraction of either the anterior or posterior tibial muscles could be provoked by the strongest induced current. Hence it was manifestly an unpromising case, but notwithstanding this, the Faradization was persevered in, and in a few days there was decided amendment—the foot did not turn in so much, and the child could walk more firmly. By the beginning of July, *i.e.*, about two months after the electric treatment was commenced he could walk a little without assistance, and the ankle was rendered more tense and fixed by the action of the current, but still no distinct contractions could be remarked.

Again he ceased to attend, but on November 2nd his mother brought him to me to show how well he could walk without aid, although a little lame from the arrest of growth of the bones, and told me that matters had been steadily but slowly improving meanwhile. The muscles were still considerably wasted, and no

contractions could be elicited by a powerful induced current, nor was it acutely felt, yet the circumference of the calf had increased by a quarter of an inch.

On making inquiry this week I learned that the child had unfortunately died of scarlatina last month; but his mother assured me that some time before his illness he could walk perfectly well, and that the affected leg was nearly as large as its fellow.

CASE II.—Martha Hyland, aged three, admitted under Dr. Barton, February 24th, 1869. It appeared that she had been neglected for a considerable time, and, on admission, was a wretched looking child. She had had measles, followed by whooping cough, and after that disease had subsided, weakness of the lower extremities remained. After a month's stay in hospital she could merely draw up her legs in bed, but was quite unable to stand or walk. Partial and irregular recovery is of frequent occurrence in infantile paralysis. Recourse was then had to Faradization, and examination showed that contractility was at a low ebb, especially in the quadriceps extensor, while the electric sensibility seemed greatly impaired, for she made no sign of pain or discomfort during the sitting. This is contrary to the general experience. The contractility of the arms was also enfeebled.

In about three weeks the adductors of the thighs and flexors of the toes responded pretty well, but the quadriceps still remained very sluggish. She was almost able to walk when allowed to lean slightly on any means of support. Voluntary power gradually increased, the current was more acutely felt, and on the 17th of May she left hospital able to walk pretty fairly with assistance, and I heard since that date that she was better.

The first of these cases was an example of the more usual form in which only one limb, and that the lower extremity, is affected, and which has been termed "monoplegia;" while the second illustrates the paraplegic form of the disease. The prognosis as regards the restoration of the palsied muscles cannot be determined with any certainty except by the aid of electric stimulation. By no other means can the precise degree of paralysis, or the extent of the default in nutrition be accurately estimated. Many authors take a most unfavourable view of the prospect of renovation of the atrophied muscles, even as to function. Volkmann goes so far as to state that the treatment of infantile paralysis, so much as any endeavour to cure or improve the actual palsy is concerned, may be

considered hopeless, and that it is very improbable that any means will ever be discovered of restoring the interrupted connexion between the nervous centres and the motor apparatus.^a

It may be that the limb never completely recovers from the severe shock that it has undergone, but the cases just related hold out, I think, some better ground for hope; and even one successful case is a sufficient answer to such a sweeping condemnation of the utility of treatment *quoad* the paralysis as that just quoted.

Three cardinal points to be remembered during the prosecution of the treatment are, that the muscles may require to be tested with a very strong current, that voluntary power may return although no evidence of muscular contraction can be elicited by electricity, and that perseverance in the means used, in the face of scant amendment and tardy improvement, is quite essential. Particular stress is to be laid on this latter proviso.

No doubt some cases recover spontaneously with rapidity, such as those described twenty years since by Dr. Henry Kennedy;^b but the experience of all who have undertaken the charge of infantile paralysis teaches us that the treatment must commonly be prolonged for many weeks or months, and that even when years have elapsed all hope is not to be abandoned.

The more completely and extensively electro-contractility is disturbed the worse the prognosis, and if the paralysis has lasted for six months or a year before treatment is commenced, very little, if any, hope can be held out.

It would be too long a question to discuss the relative merits and position of the induced, and of the battery current, in the treatment of this ill-understood disease. Each has its advocates; but in order to meet a number of cases with the greatest amount of success it is needful to have the command of either kind of current; and weighty evidence of the utility, and, indeed, necessity of the direct galvanic current has been adduced by Benedikt, Hammond, and others.

^a See an exhaustive Lecture on Infantile Paralysis and Paralytic Contractions by Professor Volkmann. *Lancet*, February 19, March 5 and 19, 1870.

^b *Dublin Quarterly Journal*, February, 1850.

ART. V.—*Case of Trephining the Mastoid Process.* By W. COLLES,
Surgeon to Steevens' Hospital.

THE experience of most surgeons and the records of periodical and general medical literature fully prove that there are few cases which excite more alarm in the mind of the practitioner than those in which, after there has been for some time a discharge from the ear, symptoms of head affection suddenly supervene. We find that the termination of such cases in general is rapid and fatal, and that on examination more or less disease of the brain or its membranes, with erosion of the bones in the neighbourhood, exists. We have numerous records of such cases and of the *post mortem* examinations but few of recovery from this state.

Having been so fortunate as to conduct such a case to a favourable termination by a proceeding often suggested but I believe seldom practised, I venture to record it, as it may lead surgeons to adopt it more freely, and thus to rescue some patients from a most unfavourable condition.

CASE.—A middle aged married lady who had previously enjoyed tolerable health, returning from England last September on the Holyhead packet, was suddenly seized with severe pain in her left ear; this continued a few days when it was relieved by a discharge of pus from the meatus; this continued, and she found the hearing of this ear gradually failing. After a few weeks she perceived a small piece of bone in the discharge, the pain soon after began to return; this, at first, was in the vicinity of the ear, but gradually increased, and at length attacked the head; it was most severe at the back part of the left side, and was such as to make her fear the loss of her reason. It would exist for two or three days, then moderate, but never leave her; she complained of numbness and debility of the side; there was also considerable constitutional disturbance, loss of appetite, thirst, quick pulse, dry skin, loss of sleep. During the progress of the disease she was alarmed by finding a sudden burst of matter into her throat; when it ceased there she felt unable to move, and any effort only aggravated her symptoms.

December 28th.—I saw her with Dr. Armstrong, her medical attendant, and found a fulness behind the ear, pain on pressure, and a blush of redness about the situation. I advised an incision down to the bone. It opened a small artery which was stopped by

pressure, the symptoms were relieved, but in a few days the wound closed up and all the symptoms returned with even increased severity. I now recommended the perforation of the bone, but she would not consent to this operation till she was again alarmed by a second gush of matter into the throat.

January 5th.—I now found the patient much worse; the swelling and redness behind the ear considerably increased; swelling extending above the ear and along the front of the face so that the sides of the head were quite unequal. She also complained of pain on moving the jaw, and a sensation as if the bones were dislocated on any motion and returned into their places with a grating sensation. As she was unwilling to use chloroform, I applied the æther spray to the part, then made an incision behind and parallel to the ear down to the bone through parts very much thickened when a drop of thick pus came away. I then used a quarter inch drill fixed in a horizontal handle; with a few turns of this I perforated the outer shell of bone. I then with a sharp steel probe broke down the walls of the mastoid cells to the depth of nearly an inch, when she suddenly exclaimed, "what is that loud noise!" On withdrawing the probe the matter began to flow more freely, and continued for some days, when a fresh swelling appeared which was caused by an abscess above the opening; this was evacuated by enlarging the incision, when the bone was felt quite bare.

The symptoms again began to mend, and she continued to improve daily—the pain ceased and the appetite and strength gradually returned. She now sleeps well, and is able to take exercise, and, more remarkable still, the hearing has returned, and is as perfect in the left ear as in the other.

This case is imperfectly recorded, as I only saw the lady occasionally in consultation with Dr. Armstrong; I may, however, make a few observations on its progress and treatment. There can be no doubt but that here the disease was very extensive, it must have occupied the cavity of the ear as evidenced by the discharge, the portion of bone expelled, the loss of hearing, and the curious symptom, hitherto, I believe, unnoticed in similar cases, the discharge of a quantity of pus into the throat on two separate occasions, which must have passed from the internal ear through the Eustachian tube. Yet with all this amount of disease we find the hearing fully restored, after its loss for so many months.

The disease then extended to the mastoid cells, as indicated by the pain, swelling, and redness in this situation, and finally, it threatened to produce cerebral mischief as indicated by the fever, severe pain in

the head, sick stomach, the numbness and debility of that side, and the great general prostration.

The treatment of such cases may be considered as medical and surgical. The use of such remedies, local or constitutional, as the surgeon may consider likely to subdue inflammation and arrest the tendency to suppuration and death of the bone, I need not now dwell upon. These were fully and freely used by Dr. Armstrong in this case, but were ineffectual, as indeed they generally are. We must then resort to surgical treatment, we are recommended by Sir William Wilde and subsequent authors to make an incision over the mastoid process, especially if we find any thickening, redness, or pain on pressure in that region; this failing to give relief, it was suggested by the late Mr. Toynbee, to perforate the cells; he does not, however, record a case in which it was attempted. In the present case, at the time of the first incision the redness was confined to the lower end of the mastoid process, and was comparatively slight; at the second this extended behind and above the ear, and even the swelling and pain had gone down towards the front of this organ; we also found the symptoms of head affection had become considerably aggravated and pain much increased. The operation of trephining the mastoid cells, though formidable in name and alarmingly dreaded by the patient, is simple and easy of execution. We make an incision through parts swollen, thickened, and painful, close behind the ear, till we come down to the bone, we will probably divide the artery, which, however, is readily controlled by pressure, no further exposure of the bone is necessary, for the finger-nail will guide the instrument to the spot to be perforated. This is done with a drill a quarter of an inch broad, set in a horizontal handle; a few turns of this used as a trephine will puncture through the entire bony wall; this opening may be further enlarged by a conical instrument with cutting edges in the sides of the cone, also set in horizontal handle, and I should recommend the opening to be made sufficiently large. The only caution necessary to be observed is to apply the instrument close behind the external ear and direct the point slightly downwards and forwards, then with a steel sharp-pointed instrument to break down the walls of the mastoid cells fully and freely.

The result of the operation in this case was most satisfactory, the patient has had no return of the severe pains in the head, she has

recovered strength and health, is able to use the jaws satisfactorily, and the discharge from the ear has ceased entirely, but the most unexpected result is the perfect restoration of hearing, after its loss for such a length of time, and after the existence of such an amount of disease in the ear and surrounding parts; had we not resorted to the operation there is little doubt the patient would have gone from bad to worse, and finally died from inflammation or abscess of the brain or its membranes.

I find in Tröltsch's *Lectures on Disease of the Ear*, 1869, "this operation has been proposed in 1770 by Jasser for the cure of deafness and was occasionally performed, till the death of a Danish surgeon from the operation, when it fell into discredit."

Mr. Hinton in the last edition of *Holmes' Surgery* says this operation has been performed with success in about twenty cases for the cure of disease of the mastoid cells, but in the section "of disease of the ear implicating the brain," I find no allusion to it as a remedy.

I have not read any of these cases, and as no information is given where to find them, I suppose many of my readers are as ignorant of them as I am, and I therefore think this case worthy of record, as encouraging the surgeon to operate even when very alarming symptoms exist.

ART. VI.—*On v. Graefe's "Insufficiency of the Internal Recti Muscles."* By H. ROSBOROUGH SWANZY, M.B, L.R.C.S.I.; Ophthalmic Surgeon to the Adelaide Hospital; lately Assistant at Prof. v. Graefe's Ophthalmic Hospital, Berlin.

THERE are three forms of asthenopia to be distinguished:—1, nervous asthenopia; 2, accommodative asthenopia; and 3, muscular asthenopia.

Nervous asthenopia is a complex affection. Its principal seat is in the retina, which becomes very sensitive to the yellow rays of light. The retina is often hyperæmic, and the conjunctiva in a similar condition. It must be treated with blue spectacles, a judicious arrangement of the light, the eye douche, &c.

Accommodative asthenopia was first described by Donders. It depends for the most part, as the reader is aware, on that anomaly of refraction termed hypermetropia, in which the antero-posterior axis of the eye being too short, a constant effort of the ciliary

muscles is necessary in order to bring the rays of light to a focus on the retina. The consequence of this continued exertion is the fatigue, on which the symptoms depend. Want of energy of the ciliary muscle may also produce accommodative asthenopia even in emmetropic eyes, as is frequently observed after long and wearying illnesses. The treatment of this form is equally well known, and consists in suitable convex glasses.

Muscular asthenopia is caused by insufficiency of the internal recti muscles, *i.e.*, a want of energy of those muscles; or it may be looked upon with equal correctness as produced by a dynamical strabismus divergens, *i.e.*, too great energy of the external recti. The result of this want of energy of the internal recti is, that convergence of the visual lines for near objects can be maintained but for short periods and with difficulty, the weak interni being unable to overcome their over-strong antagonists, the externi. A good idea of the symptoms of the affection may be obtained by any one with normal eyes, if he hold a prism with the base outwards before one or both eyes while he reads, or carries on some other close work for a time.

These symptoms resemble much those of accommodative asthenopia; while in some cases, and in some respects, they present distinguishing marks which aid us much in our diagnosis.

In both forms, reading, sewing, &c., can be carried on for some time, then similar pains and sensations of tension in and around the eye come on. In the muscular form these sensations are situated, perhaps, more in the brow than is the case in the accommodative form. In the latter, if work be proceeded with, the objects soon become indistinct; in muscular asthenopia this is also the case; but here, if the patient be reading, the indistinctness consists more in a shifting of the words and lines over each other, than in a general cloudiness of them. In both forms a shorter or longer rest enables work to be proceeded with again; but in the muscular form the benefit derived is less striking—in it the rest must be of longer duration, and the periods of work shorter. Many persons suffering from muscular asthenopia learn to cover one eye with the hand during close work, by which means they succeed in evading the troublesome symptoms. How this manœuvre has the desired effect will be seen in the following pages.

Insufficiency of the interni, however, sometimes plays another and a more serious role than that of producing muscular asthenopia, namely, in progressive myopia.

There is a popular opinion that short sight is the best sight. It rests principally upon the fact that in their old age myopic persons are enabled to do without convex spectacles for close work. And indeed, if the myopia be not of very high degree, and if the patients be moderately attentive to dietetic rules suited to such eyes, they will usually escape without any further consequences. In general, however, as Donders remarks—"A near-sighted eye is not a sound eye. In it there exists more than a simple anomaly of refraction. The optical characteristic of myopia may consist in this—the anatomical is a prolongation of the visual axis, and the latter depends upon morbid extension of the membranes. If this extension has attained to a certain degree, the membranes are so attenuated, and the resistance is so diminished, that the extension cannot remain stationary, the less so, because in the myopic eye the pressure of the fluids is usually increased. In this progressive extension progressive myopia is included, which is a true disease of the eye."

This is not the place to discuss the subject of progressive myopia, one of the saddest chapters of ophthalmic surgery; suffice it to say, that everything which tends to increase the congestion of the internal membranes, and to increase the pressure of the fluids, assists in the attenuation of these membranes, and in the prolongation of the visual axis, and increases also the liability to detachment of the retina.

A priori, we would say there could be nothing more calculated to have this effect than an insufficiency of the internal recti muscles, where every attempt at convergence (all the greater in consequence of the myopia) must be attended with abnormal muscular exertion and abnormal pressure upon the globe.

Having, during the period of my assistantship, had abundant opportunity of testing the value of the treatment to be applied in this affection, both in the hands of my great master and under my own care, I shall endeavour, in the following pages, to give a succinct, and at the same time complete account of it. I offer the less apology for so doing, inasmuch as, with the exception of Prof. v. Graefe's own clinique, I am not aware of any, where insufficiency of the interni has attained to its proper place in ophthalmic nosology. One sees, indeed, here and there, a very flagrant case under treatment, but I imagine that many of the less apparent and not less important cases escape observation. A complete account, too, of this affection is not to be found in any one paper. Von Graefe's original articles have been written at different times, and

occur in two different journals;^a and the text books, both German, French, and English, are unable, in the space they devote to the subject, to give it anything but a superficial consideration.

Tenotomy of the rectus externus, as to be described in the following pages, is the treatment to be adopted in most cases of insufficiency of the interni, whether the latter be the cause of asthenopia or be conducive to the progress of myopia. If, however, the patient be emetropic or hyperopic, I should not press him to submit to the operation, although highly to be recommended for the purpose of removing the asthenopia, for then the insufficiency carries with it but little danger for the health of the eyes. But if the case be one of myopia, I never omit to examine the dynamical condition of the lateral muscles; and if insufficiency of the interni be present, and the myopia on the increase, I always earnestly recommend the operation, even if no asthenopia be complained of, placing before the patient, if not absolute blindness, at least an extremely reduced power of vision as the probable alternative.

The result of the tenotomy in these cases is most encouraging. Of eighty cases of exquisite myopia progrediens, of which Prof. Graefe took exact notes, seventy remain stationary after the operation, four continued slightly progressive, and but six continued as progressive as before the operation.

The certain diagnosis of insufficiency of the interni can only be made by aid of an objective examination.

The coarsest test consists in causing the patient to look steadily with both eyes at a small object (the end of a finger or pencil) held on a level with his eyes in the median line; the object is then gradually approximated, and, if insufficiency of the interni be present, when it has reached a certain distance from the eyes—eight or ten inches—one of them will be seen to deviate outwards, the internus not being equal to so great an exertion of convergence while the other continues fixed upon the object. At a point, indeed, situated much further off, at one or two feet, or even many yards, an over exertion of the interni may have been required to obtain the necessary convergence, but the effort was made for the purpose of preserving single vision. The nearer, however, the object be brought to the eyes, the greater is the demand made upon the energy of the interni, and at last comes a distance for which this is no longer

^a Archiv für Ophthalmologie, Vol. ii. 1., page 174; Vol. iii. 1., pages 308-325; Vol. viii. 2., pages 314-362. Klinische Monatsblätter für Augenheilkunde, September, 1869.

adequate. At this point we often see a struggle between the tendency of the muscles and the physiological demand for single vision—now one eye deviates two or three lines outwards, but the double vision produced causes it to return to its fixation—again the debilitated internus relaxes in its effort, and the eye diverges as before, once more to recover its fixation, and so these oscillations go on for a few seconds until the useless attempt is given up, and then, one eye being abandoned to the action of the rectus externus, muscular equilibrium is restored. It will be easily understood, that in the struggle just described, a great deal depends upon the activity of the retinal functions. Thus, take an extreme case: if one eye be completely blind, the tendency of the externi will be undisputed, for then the question of double vision does not come into play; but if one eye be slightly amblyopic, or be of a different power of refraction from its fellow, the retinal images being then indistinct, a diplopia will be less disturbing, and consequently the less reason will exist for the interni to continue the contest.

Another test for the presence of insufficiency of the recti interni is the following. The patient steadily regards a small object held, as above described, at about eight inches from the eyes. Then we exclude one eye from the act of vision by covering it with a screen; as, for instance, one of our hands, or a sheet of paper; the uncovered eye continues in its position of fixation; but if we manage to get a glimpse of the other behind the screen, we find that it is deviated outwards, and when we re-admit it to its functions it has to make a movement inwards in order to fix the object once more. The reason for this deviation of the excluded eye is, that the internal recti had to exert themselves much in order to maintain the convergence for an object at eight inches, and they did so to preserve single vision; when, however, the necessity for this exertion was removed, the one eye being excluded, and there being therefore no possibility of double vision, then the eye diverged until the position of muscular equilibrium was reached.

These two experiments are, however, but rude ones, and should they give a negative result, the presence of insufficiency of the interni would not be disproved. Perfect convergence may be maintained up to within two inches of the eyes, and yet a very considerable insufficiency exist, if the physiological demand for single vision be very highly developed in the particular case. And again, if the insufficiency be but slight, the readjustment movement which takes place on withdrawing the screen, and by means of which we are informed of the divergence which has just existed, may easily escape notice.

The most delicate test, and the one which must be resorted to A sooner or later, consists in the production of crossed double images by means of a prism held with its base upwards or downwards before one eye. It is termed "the equilibrium test." In this experiment we make use of the physiological fact, that a prism, stronger than 1° or 2° held with its base upwards or downwards before one eye, produces unconquerable diplopia; in other words, that by a contraction of the rectus superior or rectus inferior few persons are able to overcome; *i.e.*, neutralize the diplopia produced by a prism of more than 1° or 2° with its base respectively B downwards or upwards. Thus, let a prism of 15° be held close before the patient's left eye with its base downwards, and he be then directed to regard—at the reading distance—a small spot through which passes an extremely fine vertical line (A), drawn upon a sheet of white paper. If, now, the lateral muscles be in their normal condition of equilibrium, the patient will see two spots on one line (B), the upper spot belonging, of course, to the left eye. If, on the other hand, there be an insufficiency of the interni present, the patient will see two spots on two different lines (C), the spot and line which stand higher, and therefore C belong to the left eye, standing also somewhat to the right (crossed double images).

The rationale of this experiment is, that by placing so strong a prism before the eye we produce a diplopia which no exertion of the rectus superior can neutralize. All further effort, therefore, on the part of the rectus internus of that eye to preserve single vision being now in vain, the eye deviates outwards, and thus to the upward displacement produced by the prism, is added the crossed lateral displacement dependent upon divergence of the optic axis.

This being the great test for the presence of insufficiency, we might now estimate the degree of the affection by the lateral distance between the two images. For many reasons, however, this linear measurement is inferior to that made by means of prisms, which is effected in the following way. Retaining the vertically refracting prism before the left eye in the above supposed case, we experimentally discover the prism which, held with its base inwards before the right eye, corrects the lateral displacement of the images, causing the spots to be situated directly over each other upon the same line. If this prism be one of 18° , we would note the case thus:—Insufficiency of the interni at 8 inches = 18° .

The object of the line in these experiments is merely to permit of the lateral displacement being at once perceived by the patient, and great care must be taken that it be very fine, as otherwise there may be a tendency to fuse the double images of the lines; indeed, in some persons this tendency may be so great, that the double images of even the finest lines become fused, and thus a wrong conclusion arrived at. This must always be controlled, after the correcting prism for the lateral displacement of the double images has been found, by means of the slightest inclination of the vertical prism, in the following way:—If one line with two spots be seen, and, so soon as the base of the prism be turned somewhat temporally, a crossed diplopia is produced, or if it be turned in the least towards the nose, homonymous diplopia results, it shows that our experiment is correct. But if these motions with the prism do not produce the corresponding displacement of the images, we may be assured there are exertions of fusion active, and the line must then be made finer or left out altogether.

The dynamical condition of the muscles ought also to be examined for distant objects by means of the equilibrium test. Here this is best done by means of a lighted candle as the object of fixation, its flame corresponding to the spot, and the candle itself to the line in the former experiments. It is placed 8–10 feet off in the median line, and somewhat below the horizontal plane in which the orbital axis lies. We generally find the insufficiency here much slighter than at the short distance; indeed, sometimes with a very high degree of insufficiency at the reading distance, dynamical equilibrium exists a few feet off.

This may be the best place to consider a factor which is of importance for the treatment, namely—what is called the voluntary or facultative divergence and convergence; *i.e.*, the amount of energy which the rectus externus or rectus internus can respectively develop in the interests of single vision.

It is found in the normal eye that the recti interni can overcome much stronger prisms with the base outwards than the externi with the base inwards; as the object is approached gradually to the eyes, this power of facultative abduction increases rapidly, but does not attain such a degree as to be greater than the facultative abduction, which has also increased somewhat, until close to the dioptric near point, about 6–8 inches from the eyes, where the externus suddenly becomes more powerful than the internus, this then being the region of muscular asthenopia for even normal eyes. In

insufficiency of the interni, however, we find the facultative abduction very much greater than normal, and the facultative abduction very much reduced. In looking at distant objects, a prism of three or four times the usual strength with the base inwards will be overcome by abduction, while a very weak prism with the base outwards will produce unconquerable diplopia. At close distances the preponderance of the power of abduction over that of adduction is still more pronounced.

We may now proceed to consider the treatment of insufficiency of the interni. In slight degrees of the affection, and if the patient be myopic, it may be sufficient to remove the near point somewhat further out by means of weak concave glasses, so that by diminishing the angle of convergence we reduce the demand upon the energy of the interni. Suitable prismatic spectacles, the bases of the prisms inwards, are very applicable to slight cases. Their use, however, cannot be extended to all cases, for when strong prisms are required they reduce the acuteness of vision, decomposing the light too much. The exclusion of one eye by covering it with a black glass, &c., can hardly be regarded as a proper treatment, although it is sometimes unconsciously adopted by the patients themselves, as has been mentioned.

Of the gymnastic treatment, by means of prisms with their bases outwards, I have myself no experience, as it has been almost quite abandoned by Prof. v. Graefe. The object is, by wearing such prisms for the distance, and by gradually proceeding to stronger ones, to stimulate the feeble interni to increased action. This mode of treatment demands great patience from the patient, and the most minute attention on the part of the surgeon; for if the prism chosen be in the least too strong, it will defeat its own object, fusion of the double images being impossible for any length of time, one eye relapses into divergence. For extreme cases, too, this treatment cannot be adopted, because then insufficiency is present even for distant objects, and therefore the weakest prism would be too strong for the interni.

Operative treatment is the one by which the most effectual cures are to be obtained.

The admissibility of performing a tenotomy of the rectus externus in these cases, when the optical axes are still properly directed to the object of vision, and the possibility of thereby correcting the insufficiency of the interni, will, I hope, be easily understood after the perusal of the foregoing. It was shown that,

in insufficiency of the interni, upon excluding one eye with a screen it deviates outwards until it reaches the position of muscular equilibrium. If, now, this divergence amount, say, to three lines, and we tenotomize the rectus externus under the screen in such a way that the bulb rolls inwards to that amount, upon removing the screen the eye will be found in its proper position of fixation, and will remain so without any excessive exertion on the part of the internus. It is, in fact, this principle which is put into practice, and it is possible to do so with the very greatest precision in obtaining the desired effect.

Inasmuch as in these cases the insufficiency of the interni for distant objects is sometimes quite absent, generally much smaller, and seldom so great as close at hand, it might fairly be objected, that if it be corrected for the latter position, an insufficiency of the externi for distance will be produced. Fortunately this cannot form a contra-indication for the operation, because distant vision is never maintained for any length of time, and consequently insufficiency of the externi in this portion of the range of vision causes no inconvenience. It would, on the other hand, be a serious mistake if, in correcting the insufficiency for the reading distance, we were to produce a real convergence with homonymous diplopia. Of this we need have no fear so long as an adequate facultative divergence for the distance is present, and this it is which gives us the measure for the tenotomy. Thus, suppose the insufficiency of 10 inches = 20° , and at 10 feet = 8° , and the facultative divergence = 20° . If we were to tenotomise the externus here in such a way as to have an effect equal only to prism 8° , in order to avoid an insufficiency of this muscle for the distance, we would still leave an insufficiency of the internus at 10 inches, equal to 12° . This would be a very imperfect result, and we are by no means obliged to confine ourselves to it. We may, much more, in this case, perform a tenotomy whose effect will be equal to a prism of 20° , *i.e.*, the whole of the facultative divergence. By so doing we restore muscular equilibrium at 10 inches, while in the distance binocular fixation will be effected with considerable exertion of the externi. In fact, after the operation the patient will see at a distance with the same exertion of his externi as he previously did when a prism of 20° with the base inwards was held before one eye. For the great good of muscular equilibrium at short distances, we exchange the very slight evil of an insufficiency of the externi at long distances. The law then is—"We find the measure for the tenotomy, by seeking

the strongest prism which at a distance is overcome by divergence."

If the "abduction prism" be less than 8° , the operation is contra-indicated, for it is not in our power to confine the effect of the tenotomy below this.

In seeking this prism we must be careful lest we should choose too weak a one, and still more so lest we should choose too strong a one. By attention to the following points these errors may respectively be avoided. If the power of abduction in proportion to the disturbance of equilibrium, as found by the equilibrium test, appear very small, and below the amount (8°) required to admit of operative interference, it is then often desirable to prescribe the abduction prism overcome, to be worn by the patient for the distance for some time. Frequently then, after a few hours, the power of abduction increases by 2° or 3° , and after two or three days by 4° or 5° , and then we obtain a facultative divergence of the amount required, its absence in the first instance being due alone to want of practice. Care must be taken that the double images stand each in the same horizontal plane. If they do not, it may be accomplished by a slight rotation of the prism, and then the facultative divergence will be found greater than was supposed, one image standing higher than the other having previously made fusion difficult. This difference in height may have arisen from the surgeon not having held the prism strictly horizontal; sometimes, however, in these cases, besides the disturbance in the lateral equilibrium, there is one in which the recti superior and inferior are concerned, causing the difference in height of the double images. Care must be taken that the patient does not exclude* under the prism—a circumstance which might lead us to estimate the facultative divergence too highly. To be assured against this error it is not sufficient that with the least rotation of the abduction prism diplopia appears, because patients who are in the habit of periodically excluding, as is often the case, find it easy to do so for images which lie in the same horizontal plane, but not when a slight difference in the height of the images is induced; nor is it sufficient that the next strongest prism produces homonymous

* Voluntary "exclusion" or suppression of a double image plays an important role in the general subject of strabismus. It is purely a psychological process, and must not be confounded with amblyopia—the eye in question possessing often perfect power of vision. Emile Javal (*Du Strabisme dans ses applications à la physiologie de la vision*. Paris, 1868), likens it to the power we possess of listening to a conversation in which we are interested, while another, carried on close at hand, passes unperceived.

diplopia, for it often happens that for certain eccentric positions the patients exclude, and not at either side of this. The only method of finding if the patient excludes with one eye is to cover each successively with a screen. If on doing so the covered eye remains in the same position as previously, we may know that there is no exclusion active; if, on the other hand, one of them then makes a slight rotatory motion, the contrary is true.

The tenotomy of the externus is to be performed on that eye which first deviates outwards on approaching the fixation object, or it is to be performed on the eye with the strongest facultative divergence, or on the one with the worst acuteness of vision.

The proportioning of the operation to the effect desired is a most important consideration, and the surgeon who ventures to undertake the proceeding without being perfectly at home in the mode of executing it, will, most probably, instead of bettering the condition of his patient, visit him with the most disturbing diplopia. I have seen many melancholy instances of this in persons who had been operated upon in other parts of Germany and in Russia. Professor von Graefe has laid down the following rules for proportioning the effect of the operation, founded empirically upon his own vast experience.

A simple tenotomy of the externus produces an effect equal to that of an abduction prism of 16° —accordingly with a facultative divergence of 15° , 16° , 17° , or 18° , a simple tenotomy is performed. For one of 14° and less a restricting suture must be applied. This suture, by drawing the edges of the conjunctival wound together, prevents the too great retraction of the muscle. Its effect depends on its direction (horizontal), on the amount of conjunctiva it contains, and on the closeness with which it is tied.

If the abduction prism = 14° or 13° , a slightly horizontal suture is to be applied, containing on each side one line of conjunctiva, and it is to be tied so as barely to close the wound. If 12° or 11° , then a moderately horizontal suture, containing about one line on either side, the wound to be closed. If 10° , a moderately horizontal suture, containing about one and a-half lines; the wound to be closed. If only 9° or 8° , the suture is to be applied as horizontally as possible; is to include two lines of conjunctiva on either

* V. Graefe never draws the edges of the conjunctival wound together after a strabotomy for the purpose of producing a better union of these; for, unless they be precisely adjusted the proceeding is of no use, and this he believes to be impossible in such a tissue.

side, and the edges are to be drawn tightly together. If the abduction prism amount to more than 18° , it is best to divide the operation between both eyes.

By attention to the above rules the operator will generally effect his object; but not always, for there are individual differences in the elasticity of the muscle, and in the degree of yielding of the conjunctiva, which cannot be calculated on before hand; and should a hemorrhage take place beneath the conjunctiva, it would increase the effect of the suture considerably. Immediately after the operation, therefore, it is necessary to control the effect by means of the equilibrium test, made in what is termed the "position of election."

The position of election lies about 10 feet from the eyes, about 15° towards the side of the unoperated eye, and 15° below the horizontal plane in which the orbital axes lie. Experience shows, that in this position the immediate effect and the ultimate effect bear a much more constant relation to each other than in the median line. In the latter place the transitory operative effect of the tenotomy comes too much into play, and a considerable convergence in this position immediately afterwards might lead the surgeon to think he had done too much, whereas he may, in fact, not have done enough. The transitory effect continues in a greater or less degree until the muscle becomes perfectly re-attached.

In order to obtain the correct ultimate result the rule is, that muscular equilibrium must exist in the position of election immediately after the operation. A dynamical divergence of 1° or 2° may still be looked upon as correct, and if the measure of the operation was high (15° or 16°) a convergence of a similar amount may be allowed to remain—otherwise not. If the equilibrium test does not show a condition in accordance with these rules, an immediate alteration of the suture in a corresponding direction must be made.

With attention to these rules a real convergence with diplopia in the median line may be avoided, but they do not afford protection against temporal diplopia towards the operated side, and to avoid this the surgeon must at the same time control the defect of absolute motion which has been produced. A defect of motion towards the operated side of more than 3 lines must never be allowed to remain, but must be moderated by a supplementary suture. If the measure of the operation was high (14°), a defect of $2\frac{1}{4}$ — $2\frac{1}{2}$ lines may be left; if the measure was less than this a

defect of motion of even 2 lines must be moderated. A defect of $1\frac{1}{2}$ lines may be left, even with small measures of operation (8° – 9°) without any danger, always provided the demands in the position of election are supplied. But the correction of too great a defect of motion may influence the result of the equilibrium test, particularly if the measure of the operation was high. In such cases the surgeon may be obliged to attain his object by another operation on the other eye, or he may prevent the dilemma by previously determining to divide the effect between both eyes.

If the condition be examined 6 or 7 hours after the operation, a slight increase in the effect will be found in the position of election. Instead of dynamical equilibrium slight (1° – 2°) dynamical convergence will be found. This convergence increases until the fourth day, when it amounts to 3° , 4° , or 5° , and in the median line to 8° – 16° . According to their greater or less power of fusion, the patients see double on the other side of 4 or 3 feet, or even $1\frac{1}{2}$ feet in the median line. This increased effect decreases again from the 5th, 6th, or 8th day, so that the diplopia in the median line disappears, mostly in the 2nd, or at latest in the 3rd week, and the lateral diplopia to the side of the operated eye some weeks later. The defect of absolute motion usually remains the same for some days after the operation, and then gradually decreases until, 2 or 3 weeks afterwards, it only amounts to half, and in some months later the quarter of its original amount.

The after treatment. An immobilizing bandage is to be placed on the operated eye to promote the reattachment of the tendon. The patient should be visited 6 or 8 hours after the operation, and the equilibrium test made in the position of election. As above-mentioned the effect will generally be found somewhat increased; but it may be found that the effect is increased more than is allowable (5° or 6°). This sometimes depends on the suture having come out, but more frequently on the conjunctiva being very delicate, and having become torn at the points of puncture of the suture; sometimes it may be that an ecchymosis having been present has become absorbed, by which the effect of the suture has become decreased. Under any of these circumstances the old suture must be removed, and a new one put in, which will restore equilibrium in the position of election. At this time one seldom finds the effect reduced; if it be so, it may be consequent on an ecchymosis which has occurred after the bandage has been applied, or on an inflammatory swelling of the portion of conjunction enclosed in

the suture. If the decrease of effect be only moderate (2° or 3°) it may be disregarded, particularly if the measure of the operation was low. If it is great (5° or 6°), it must be corrected, and if then the measure of the operation was small, it may be indicated to leave away the suture altogether. Twenty-four hours after the operation the effect is still more increased, as above mentioned. By this time the suture having established its effect, it may be removed; in case of very nervous patients, however, it is well to leave it another day, particularly when the increase of the effect is very pronounced, lest in removing it the delicate adhesions be loosened, and the effect permanently increased. If at this period, even by tearing out of the suture, &c., a too great increase of the effect be present, a new suture must be applied, at the same time that, by gently pulling at the edge of the conjunctival wound, the adhesions are somewhat loosened. If, on the other hand, the desired increase in the effect of the operation be not present, the suture must be removed, and the patient made to look to the side of the unoperated eye, by which means the still delicate adhesions will be stretched, and our object attained.

On the second day pretty much the same principles are to be observed. If there be dynamical convergence of 2° – 4° in the position of election, there are no measures to be taken. If dynamical convergence of 7° or more, a new suture, combined with loosening of the adhesions, is to be recommended. If there be dynamical equilibrium, then direct the eyes strongly to the unoperated side. If there be dynamical divergence, and the suture be still in position, remove it, and apply a horizontal suture on the inside of the cornea, containing a large fold of conjunctiva, to increase the effect.

If in the next few days (3–6) the condition deviates from the rules laid down, then, in case it be necessary to increase the effect, the patient must be made to look persistently to the opposite side. On the other hand, if it be wished to reduce the effect, it cannot be done by directing the eyes to the side of the operated muscle. If the increase of the effect be not greater than 5° or 6° , and everything has been previously calculated correctly, it can quite safely be left so. If the increase be greater than this, the only remedy is to raise the temporal edge of the wound, and with a small strabismus hook having separated the attachments, to apply a new and more effective suture; but it will be seldom that such extreme measures as this are necessary.

In the second and third week, if the proper decrease in the effect does not appear, the associated movements may now (from the 8th day on) be directed towards the side of the operated eye with good result. If the patient begins to read towards the end of the second week, the strongest possible abduction prisms are to be prescribed; if it be desired to reduce the effect, whilst at the same time the just mentioned exercises are practiced. But if in this and later periods, it is desired to increase the effect, abduction prisms, combined with the concave glasses indicated, are highly to be recommended for distant vision.

ART. VII.—*A New View of the Origin and Propagation of the Venereal Disease—Successful Treatment by Inoculation, derived from a hitherto Unknown Source, with Illustrative Cases.* By J. MORGAN, F.R.C.S.I.; Professor of Surgical and Descriptive Anatomy, Royal College of Surgeons, Ireland; Surgeon to Mercer's and the Westmoreland Lock Hospital, Dublin.

No disease has, perhaps, been more anxiously discussed by the profession than Venereal, as from its peculiar nature its effects are so varied and its history is so frequently obscured, or even purposely rendered difficult by the patient's self; while to the state it is a subject of no mean importance, from the injury done not only to our Army and Navy, whose efficiency is thus materially impaired, but also to the succeeding generation of those who may suffer from its wide-spread poison. Epidemic diseases, though their immediate effects are limited fortunately to the individual, by their startling incidents and subtle effects at once rouse both the profession and the community to limit and combat their extension; but this poison is constantly and occultly disseminated and propagated not only to the recipients, but to their offspring. The legislature will in time, no doubt, without partiality discuss the effects of the Contagious Disease Act so far as it has gone, and prove, despite prejudices, and by the stern force of practice against theory, how this Act has absolutely worked, and whether it has been effective even in its limited operation in restraining the extension of the disease.

Amongst the more prominent questions arising from an examination of the subject is the oft-debated one of the nature of the venereal poison, and if any light can be thrown on the

variation that has been observed in its influence on different individuals, some of whom suffer so severely, while with others its effect is transitory and apparently harmless. To aid in this object I propose to limit myself to actual practical observations and deductions from the large number of cases which I have had the opportunity of observing in the wards of the Lock Hospital, amounting to a total of 1,582 for the last two years, and to indicate a new source of contagion from which the usual soft or chancroid venereal sore is derived, as proved by numerous direct inoculations and experience. The origin and source of this contagion is the more interesting, since so much has been written and discussed as to its infecting or non-infecting nature, and as to its origin from a hard or "infecting sore," or a sore *sui generis*, and whether it may or may not be the initial point of true constitutional infection, and of the long and obstinate train of symptoms that not unusually supervenes.

Notwithstanding all that has been contended with regard to the nature of the venereal poison, and the theoretical views entertained as to its forms, or as to the existence of a special virus, yet, as the result of daily observation, I must record the remarkable fact that at this hospital *all* cases of primary sores, as a rule, are sooner or later followed by constitutional signs, often before the patients leave the hospital, and frequently before the healing of the primary affection. During the last year out of all my cases, whether tested by inoculation or otherwise, I have been able to find two only that, so far, have not shown decided evidences of infection. With us in Dublin it is evident from the hospital statistics that the poison predominating in the class through whom the disease is propagated is of a truly infecting character, and of an inveterate type; every day's experience confirms me in this view of the affection in this city, and army surgeons have frequently testified to the virulent nature of the disease contracted by the troops under their charge when stationed in Dublin, as compared with other military stations throughout the country. The last Metropolitan Police Report gives the number of women known to the police in—

1865 as 1,078.

1866 „ 1,031.

1867 „ 1,047.

1868 „ 969.

These figures only embrace such females as are well known; and it may be readily granted that they represent very imperfectly the

actual numbers. As the Westmoreland Lock Hospital is open to those who only voluntarily enter, and 805 admissions for various forms of contagion were registered in the hospital books this year, some idea may be formed of the immense amount of disease prevalent in Dublin amongst this class so dangerous to the community; a great number of whom seek medical relief at the various public institutions, and at the same time are daily communicating disease, and all our Hospital or Dispensary Surgeons can testify to the large number of male venereal cases which come under their notice.

The discrepancy amongst syphilographers as to the variations in the forms of the disease and its after consequences in the male and female, always seemed difficult of explanation, and led me to make a close examination by experiment as to its nature and origin in the patients under my charge at the hospital as compared with male patients met with in private and general hospital practice. I could not fail to observe that in this city, the frequency of chancre or soft sores in the male uncomplicated with constitutional signs, was excessive in proportion to those cases in the female who suffered from genital sores, while on the other hand the frequency of constitutional symptoms in the female was excessive in proportion to the male. Some reason I had no doubt existed for these peculiarities, either in the transmissibility of sores in diverse forms, or that other sources of contagion existed than in the sore itself.

The frequent coexistence of a vaginal discharge of a mucopurulent or purulent form with the earlier stages of constitutional syphilis, afforded, I thought, some clue to the solution of this question, and led me to make some experiments, the result of which I have shown in the series of cases which I tested. By those it appears that the product of the vaginal discharge of a patient suffering from syphilitic infection, is a chancre or soft sore, when the discharge is introduced under the skin or applied to an abraded surface. It is also shown that this sore is propagated as a chancre or soft sore, and is again (so far as my experiments went) capable of indefinite propagation still as a soft sore or chancre. This is amply proved by the case related in Series No. 1, where, after the expiration of two months' confinement to the hospital—the mucopurulent and apparently innocuous discharge of a patient—not showing even at that date any evidences of constitutional infection, was capable when inoculated, of producing the well-marked chancre of Series No. 2, from which in its turn, I inoculated several other patients, producing always a chancre in appearance. The

patient from whose secretion this chancroid was produced, very shortly afterwards showed abundant and unmistakable infection signs while still in the hospital.

A more remarkable power possessed by this vaginal secretion is the production of a chancroid by inoculation on the patient's own person; the fact is interesting when demonstrated so evidently as in Series 3. Here a chancroid was produced in an intensely syphilized patient from her own vaginal discharge; from this another was produced on the patient herself, and from this product contagion was obtained (and could be obtained, I have no doubt, to an indefinite extent), having all the characters of a chancroid. It is remarkable that the primary sore from which this patient was suffering was itself incapable of auto-inoculation practised on two occasions, one previous to the formation of the artificial chancroid, and the other afterwards. The muco-purulent vaginal discharge seemed to be the true source of this contagion virus.

I have on several occasions taken the secretion as wiped from the os uteri and inoculated unsuccessfully with it, while from the vaginal discharge I have been successful.

I have inoculated from the vaginal secretion of cases of uterine ulcer and from the ulcers themselves without any result.

I have also inoculated the gonorrheal discharge of the male without result.

In all my inoculations the result has invariably been the characteristic pustule and soft or chancroid sore, capable of reproduction; the question therefore arises, was the original affection a chancroid or soft sore capable of infecting the system, or a hard sore, which is admittedly so capable? I cannot give any information on the latter point, as unfortunately in the only two cases of hard sore which have been admitted to the hospital during the last eight months I did not inoculate. So as to be capable of transmitting a sore by inoculation from the vaginal secretion it seems necessary that the patient must be suffering from the earlier stages of constitutional infection, whether as yet latent or developed; thus, in Series 1, at the time of producing characteristic inoculation, the patient had no evidence whatever beyond a suppurating bubo that seemed to be a "*bubon d'émblée*," as there was no sore discoverable on frequent speculum examination, yet shortly afterwards marked constitutional infection signs appeared. In the other cases I have given, constitutional evidences

were abundant, and, as in Series 3, were intensely marked indeed, yet the resulting sore was the characteristic and inoculative chancroid.

That the vaginal secretion retains this power for a considerable time after the healing of the primary sore or the appearance of the first constitutional evidence is amply proved by Series 3 and 5, where, in even three months after the first symptoms the specific inoculation could be performed. With regard to the transmission of the constitutional poison under the various form of sores, be they soft or hard, Ricord himself, in his "Lectures on Chancre," though at the time an avowed advocate of the dual virus, remarks, p. 144:—

"You see, gentlemen, that these facts agree, and cannot leave any doubt on the infecting character that the chancre with a soft base (*chancroid*) can, under certain circumstances, assume, when developed on a previously infected individual. It therefore now seems proved, contrary to former doctrines, that a syphilitic subject who contracts a fresh chancre (*alluding to chancroid*) may transmit the syphilis."

Here it is admitted that a fresh chancroid on a syphilitic subject is capable of transmitting the syphilis, how much more reasonable is it to suppose that, as in Series 2, the product of an already syphilitic subject derived from another also syphilitic, would, if communicated to the virgin soil of an uninfected subject, produce a chancroid in appearance, yet be infecting in its property.

Again, in Series 3, where the second generation of the seed sown and matured in the parent soil of a thoroughly infected subject, was capable of propagation as a chancroid, it is still more reasonable to suppose that the result to a previously untainted subject would be the appearance of a chancroid, but accompanied by thorough contamination, unless, indeed, we believe that some peculiarities of constitution in the recipient were capable of modifying the poison.

My inoculations have been always made on previously infected subjects, as I did not, of course, feel myself justified in directly inoculating from others already undoubtedly tainted. I therefore cannot prove by actual experiment that the result of such inoculation in an untainted case would be the chancroid in appearance, though having the contaminating virus of the true infecting sore. For my own part, from seeing instances of soft sore in the male followed by constitutional signs, I find no difficulty in assuming this to be the case.

As to the question of the origin of the soft sore of infecting character, Ricord also remarks:—

“We now have to elucidate the following question:—Does the soft chancre of a syphilitic patient, which is capable of transmitting syphilis to a healthy subject, necessarily derive its origin from an indurated chancre, or does some special, though still *unknown condition*, exist which invests the soft chancre developed under these conditions, and *what ever may be its origin*, with the infecting character which only appertains to the indurated sore.”

He adds—“There already exist a certain number of observations which tend to prove that the soft chancre of a syphilitic subject may also transmit itself in its own species, that is to say, as a soft chancre.”

In all the inoculations I made the products from the artificially generated chancroid was a chancroid, though generated from and on syphilitic subjects.

With regard to the propagative property of the poison in women, it might be supposed that once the sore is healed there is no further danger of a sore being communicated, which even a chancroid, as I believe it might appear in an untainted subject, would be lethal to the system by conveying the syphilitic poison. This error the illustrative cases show fully would be a serious one, as the non-existence of a genital sore in the female is no absolute guarantee of the non-existence of contagion power.

Series 3 proves this very definitely. On the 23rd May the first auto-inoculation for her vaginal secretion was performed; and on June 20th, twenty-eight days after the first inoculation, and from two to three weeks after the healing of the primary sore, I successfully inoculated Case 1,060 from the vaginal discharge, producing a characteristic and vigorous chancroid pustule about three and a half months after her first perception of the primary sore. This is to be borne in mind, with respect to the conduct of inspections under the Contagious Diseases Act, as it might be supposed that freedom from primary or actual sore obtained freedom from contagion, yet a vaginal discharge in a syphilitic patient might be overlooked. I am rather inclined to believe that one reason for the comparative frequency of the occurrence of soft or chancroid sores on the continent, and where Governmental supervision is exercised, is due to the fact that primary and external sores are at once detected on inspection, and the patient is confined to hospital till cured—but that the vaginal discharge is overlooked, or its discovery guarded against by the patient.

It by no means follows that this secretion need be very purulent in its appearance. In Series 1, the secretion was a muco-pus, also in Series 3; and on microscopic examination contained a vast quantity of epithelium, with pus globules scattered here and there. As it is to the pus cells that the contagious property of the disease is due, the small admixture of these with the vaginal mucous explains, I think, how it is that lapses or failures are apt to take place in the practice of inoculation from this source; and also how frequently, no doubt, contagion is not communicated in coïtu, even where there is the favourable opportunity of an accidental breach of surface, and point of entrance for the poison. As to the frequent co-existence of a vaginal or apparently gonorrheal discharge, with secondary symptoms, I have received from the Government Lock Hospital, Cork, valuable evidence. Thus, of 326 cases treated in the hospital since June 15, 1869, "about 100 were cases of sores, the remainder 226 were cases of gonorrhea, several of which were complicated with the various forms of secondaries—the majority of the complications being mucous tubercles on and around the vulva."

This is very frequently the case in Dublin also, the appearance of the mucous tubercles being invariably followed by rash, pains, and other constitutional evidences; and as in Series 4, where inoculation from a patient (No. 1,093) so affected was performed. the discharge, *or gonorrhea*, as it might be called, was capable of producing the characteristic chancroid pustule when introduced under the skin.

The question of mediate contagion, though it may be possible, is, I think, much more due to the inoculability of the discharge than to the fact of an inoculable foreign poison, which must necessarily be in exceedingly small quantity, being retained in the canal. In the experiments of Cullerier, where, after the introduction of some inoculable poison on a spatula, auto-inoculation was successfully performed, it is not easy to imagine that the *very* particle that had been introduced thirty-five minutes previously, and where the patient was compelled, meanwhile, to walk about, could be again obtained when the great extent of the vaginal mucous membrane surface, is considered, which was, as described at the time, "red-dened, and smeared with an abundant muco-purulent secretion."

One of the patients was suffering from inguinal ulcers, and vaginal discharge, just as that mentioned in Series 1, having undoubtedly syphilis; yet in the latter the inoculation produced a

true chancroid, without mediate contagion, while in other instances it failed—as where the negative inoculation was performed by M. Cullerier, previous to the introduction of the mediate poison.

The case of mediate inoculation is thus given by M. Cullerier:—

“A woman entered hospital with inguinal ulcerations, and considerable surrounding inflammation. The vagina was reddened, and smeared with an abundant mucopurulent secretion, but its mucous surface was intact, and os uteri healthy.

“M. Cullerier collected on a spatula a considerable quantity of pus from the inguinal ulcers, and deposited it in the vagina, having previously inoculated without success from the vaginal secretion. The patient was then directed to walk about, under surveillance, for thirty-five minutes, when some of the fluid found in the vagina was inoculated on her thigh, producing in two days a specific pustule.”

In this experiment there is no proof whatever adduced that the inguinal ulcers were inoculable at all. To be perfect as a trial, they should have been tested, and as it is not said they were the sores left by suppurating bubos, the question is an open one if they were really inoculable. The explanation is, I think, more likely, that the vaginal discharge was inoculable.

I have found the following variations in the conduct of these inoculations:—

The activity of the discharge seems to vary—thus, immediately before or after the menstrual period it is less active.

The same discharge would succeed in some cases and not in others, and though failing to produce a specific pustule one day it would on another.

As in vaccination, the admixture of a little blood in making the inoculation generally rendered it nugatory.

The more robust and vigorous the patient, the more perfect was the inoculation, and the more persistent it became, being indeed difficult to heal.

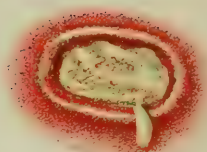
Every succeeding generation from the vaginal discharge seemed progressively to increase in inoculative power.

The more infected the recipient the more difficult to inoculate, and the greater the tendency to heal.

Every succeeding inoculation on the patient's own person seemed to increase in inoculative power.

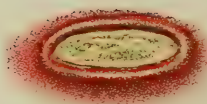
The following experiments, carefully conducted, prove the inoculability of some vaginal discharges, and the production of chancroid or soft non-infecting sores, so called, from this source,

SERIES
1



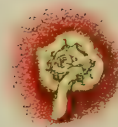
CASE
1,024

SERIES
2



CASE
1,140

SERIES
3



CASE
988

OLDHAM, fecit

SERIES 1.—Sore produced by inoculation from another sore, itself produced by inoculation from a suppurating bubo as the only lesion.

SERIES 2.—Sore produced by artificial inoculation from the vaginal discharge of Series 1.

SERIES 3.—Sore produced by artificial inoculation from another sore produced from vaginal discharge.

from patients already syphilized, upon others also thoroughly under the influence of the poison:—

SERIES No. 1.—*Inoculation with Vaginal Muco-purulent Discharge from a Patient not having a Sore, performed on a girl Eight Years old, suffering from Vulval Condylomata—Production of a Typical Soft Sore on the Abdomen—Propagation of Chancroids from this Sore—Auto-inoculation from a Bubo—Re-inoculation from this source.*

CASE No. 1,024—Ward 2—aged eighteen, a very well conditioned girl, having only left her home in the country six months before, was admitted on April 2, 1870, suffering from a very large suppurating bubo of the left groin, rather below Poupart's ligament, but having no sore, and not being aware of ever having had one. I opened the bubo, which was painful, and contained a large quantity of pus. After a fortnight a speculum examination was made, but no sore discovered; but there was some vaginal discharge of a semi-purulent character.

May 6th.—A sore was observed forming on the abdomen opposite to the orifice of the bubo, which was still unclosed; this sore in a few days assumed all the characteristics of a soft sore or chancroid. By speculum examination, repeated on ten different occasions, no sore, either vaginal or uterine, could be discovered.

On the 19th of May, 1870, I inoculated the right side of the abdomen from the naturally auto-inoculated sore, caused by the irritation of the discharge from the bubo; this formed a most characteristic pustule with depressed centre; it then ulcerated, and finally presented the aspect shown in the drawing taken by Mr. Mackay on June 14th. All the characters of a chancroid are well seen—the peculiar edge, irregular surface, and copious discharge; this sore is now twenty-six days old since the date of my inoculation.

From this sore I inoculated on the abdomen, Case 1,093, on June 13th, producing a specific pustule and ulcer of a well-marked character.

Thus far the history is clear. First—Of the discharge from a bubo causing a sore by apposition. Second—Of an inoculable sore procured from this source. Third—Of the transmission of this sore, the second generation of a suppurating bubo, to another person already syphilized, producing a characteristic pustule and sore as the third generation.

The most interesting and startling point with regard to this case is, however, the inoculability of the vaginal discharge, which I proved thus on a girl in a separate ward.

Having had the patient under my observation, and confined to

hospital for exactly two months; having by frequent and careful speculum examinations satisfied myself that there was no possibility of an intravaginal sore, and that but a very little muco-purulent discharge alone existed. On the 2nd June I removed some of the vaginal secretion through a speculum, taking care that it did not even touch any external part, and I inoculated with it on the abdomen, a girl eight years old, No. 1,140, suffering only from vulval condylomata, with a very clean and clear skin; this produced a most characteristic pustule, and became on the fifth day a perfectly formed ulcer.

On June 11th, *i.e.*, nine days after the inoculation from the vaginal secretion, I showed this girl at the Obstetrical Society of Ireland. The drawing taken on June 14th (twelve days after inoculation) shows well the similitude to the artificial and directly inoculated sore of No. 1,024.

On June 14th, the sore caused by the bubo inoculation was quite healed, having existed about thirty-five days. Papulo squamous rash also appeared on the arm, the chest, the thighs, and the loins; the hair commenced falling out, and a slight irritic attack showed itself. There is still a general murkiness of skin, but the health is immensely improved; the second inoculated sore shows as yet but little inclination to heal.

June 9th.—I inoculated from the vaginal mucus, case No. 1,158, not suffering from any constitutional signs whatever, but with a negative effect.

On June 10th I inoculated also from the vaginal secretion, which did not appear purulent, case 1,093, suffering from a copious rash and patches, but with negative effect.

On June 13th I inoculated the same patient, No. 1,093, from the artificial inoculation, and produced a characteristic pustule.

The chancroid in this patient, that was artificially produced from the first sore, which was the result of the spontaneous inoculation from the bubo, was much the most obstinate in its character and most marked in its appearance.

The patient at the date of this report, June 25th, is still in hospital, and improving rapidly.

SERIES No. 2.—Chancroid Grafted on an already Infected Patient from the Vaginal Discharge of a Patient without a sore, and who, though not at the time showing Infection Signs, did so while in the Hospital—Communicability of this artificial product.

No. 1,118.—A girl eight years of age, was admitted May 18th, Ward 8, suffering from condylomata of the vulva and anus; there was no rash

whatever, but she had an ulceration of the arm of a gummatous character. The history of the case points to her having been infected by her sister, a married woman, who had been in the hospital under my care this year, for very copious mucous patches of the mouth and labia. Her husband, to whom she had been married but a few months, I saw also; he was suffering from mucous patches of the mouth, and admitted having had a sore and suppurating bubo five months before his marriage.

This girl I inoculated on June 2nd with the vaginal secretion of No. 1,024, Series No. 1, taking it carefully by means of a speculum, as before mentioned. A most characteristic pustule and sore was produced. The appearance, as shown in the illustration, exactly represents a chancroid or soft sore of a large size.

There was no possibility of any accidental contact with a sore, as the part was carefully guarded by a watch glass, and the progress of the inoculation till the formation of a perfect typical sore was most regular. The drawing was taken twelve days after the inoculation.

In order to test the specific nature of this inoculated sore, on June 12th I introduced some of the discharge from the surface on the point of a pin on the right side of abdomen of case 1,085, a woman suffering only from a primary sore, from which I had successfully inoculated herself two weeks before on the left side of the abdomen, and produced a most characteristic pustule and sore. Still further to try the communicability of this artificial sore, I tested the patient No. 1,000, suffering from intense syphilitic poisoning, a most abundant rash, and recently having had a suppurating bubo.

On June 10th I inoculated on the left side of the abdomen.

On June 12th I inoculated on the right; both of these produced deep characteristic pus-secreting sores; but the true pustular stage was imperfectly marked in this patient.

On June 12th I inoculated 1,159 from the artificial sore, producing a well-marked chancroid with the pustular stage also hardly marked.

It is evident that in these two instances the artificially produced inoculations were equally contagious; the one, Series No. 1, produced from the offspring of a suppurating bubo in the same person, and the other, Series No. 2, produced in another person from the vaginal muco pus; these sores could, of course, be reproduced indefinitely. Supposing that either of these or similar cases communicated a sore to a male, I cannot doubt it would have every resemblance, as shown in the illustration, to a soft or chancroid sore so called; but I also cannot doubt it would be an infecting sore, having originated in the 1st series from the vaginal muco pus only of a patient showing, subsequently, undoubted signs of infection; and in the 2nd series having a double contamination, first, from the source of origin, the vaginal muco pus of an infected patient, and then

of the already infected patient, on whom the poison was grafted, and, as in this instance, already suffering from constitutional evidences. Experiments go far to explain simply how it is that constitutional evidences have been seen to follow soft or chancreoid sores.

The health of this girl rapidly improved, and the condylomata healed, but the artificial chancreoid still exists, July 6th.

SERIES NO. 3.—Inoculation from the Vaginal Discharge of an Intensely Syphilized Patient, produced on her own person 63 days after admission—Auto-re-inoculation from this artificial sore—Inoculability of this second product—Non-inoculability of the Primary Sore—Suppurating Bubo—Papular Rash—Nocturnal Pains—and Intense Cachexia—Rapid Improvement of the Health subsequent to Inoculation.

In Series No. 2, the development of a contagious sore from inoculation with a vaginal discharge is sufficiently evident, where the vaginal secretion was inoculated on a different person. A more interesting and curious circumstance with regard to communicability is that of the formation of a contagious sore on the patient from her own vaginal discharge, and the capability of this sore, so produced, to give birth to a second, and this of being propagated indefinitely.

Case No. 988, admitted March 21, 1870, aged hardly 18 years, left home in the country about four months before her admission; during that time she got a severe inflammation, apparently of a strumous nature, from which she lost the eye. About three weeks previous to admission she observed a sore on the left labium—not painful, pus-secreting, and equalling a florin in size, without the slightest induration; a painful bubo was formed in the left groin, which suppurated shortly after admission, and from which I produced a characteristic, but feebly marked pustule and sore on the abdomen.

In four weeks after her admission a copious coppery roseolar and papular rash appeared; she became intensely cachectic, racked with pains, had sweatings, and was thoroughly prostrated by the syphilitic poison.

She was treated up to this by tonics, good diet, iodide of potassium, and iron; the stomach became very irritable, and it was impossible to give any internal medicine, even a creasote mixture was intolerable.

I therefore used the cautious and careful inunction of 10 grains of mercurial ointment every afternoon. Under this treatment, after ten days, there was some improvement in the general symptoms.

On the 23rd May (being sixty-three days from her admission), I had the

patient, who was extremely weak and prostrate, carried to the table, and on speculum examination, found a rather copious vaginal muco-purulent secretion. I removed some of this through the speculum, and washed out and examined the vagina and uterine neck; there was not the slightest breach of surface, or even irritation; I inoculated the left side of the abdomen with this pus, and produced a very well marked, but not large, characteristic pustule.

On May 28th, in order to test the inoculability of the primary sore, I collected from the surface a copious charge of pus, with which I inoculated the abdomen, but with a negative result.

On June 3rd, being ten days from the date of the inoculation from the vaginal discharge, I inoculated the right side of the abdomen from the artificially formed sore, and produced a more perfect and vigorous pustule than in the first instance. This I refreshed on June 9th with her own vaginal discharge, and the drawing taken on June 16th shows well the specific appearance of this sore, which poured out copious pus, and from which I performed the following inoculations, amply proving the contagious property of this second transmitted product of a vaginal discharge on the patient's own person while suffering from intense syphilitic intoxication.

June 7th, being four days after the introduction of the second virus, I inoculated Case No. 1,075 with the pus from this second inoculation, having first allowed it to dry. Whether from this cause, or the early stage of the pustule, or some accidental mal-performance of the inoculation, it did not succeed satisfactorily, fading away in three days.

June 9th, I inoculated from her vaginal discharge Case 1,159, already syphilitic, but with ill-formed result, fading in three days.

June 10th, three days afterwards; inoculated a weak cachectic patient, No. 1,163, suffering from copious papular rash, and produced a characteristic, small, well-shaped, pus-secreting ulcer on the abdomen, the exact resemblance of the parent source.

June 11th, I inoculated with the same No. 1,169 (suffering from both primary sore and copious rash), having first allowed the pus to dry, but possibly from this cause, did not succeed satisfactorily, the pustule being abortive.

June 12th, I inoculated with the same, Case No. 1,112, suffering from what might be termed a true typical sore on the right labium, but without induration, with somewhat indurated inguinal glands on the right side, and copious papular rash appearing at the same time. This inoculation was most successful—producing a pustule and marked chancreoid; of which the illustration is very perfect, as taken two days after the inoculation, showing the pustule and peculiar inflamed base.

This series shows the inoculability of a vaginal discharge on the *patient's self*; the re-inoculability of this artificial sore on the *patient's self*;

the propagation of this sore to other patients suffering from syphilitic manifestations, as No. 1,112 and No. 1,163.

The improvement of this girl's health from the period of the inoculations was most remarkable. She is now, June 18th, almost without a symptom, though taking no medicine. Wine stimulants, good diet, have been freely given.

On June 23rd, I again inoculated her from her own vaginal discharge, and produced by the 28th a regular characteristic pustule, being now three months after admission to hospital. On the same day I inoculated from the same source, No. 1,060, with success, and No. 1,093 without success.

During the last two years no more intense case of true primary syphilitic intoxication has been in the hospital, and, though contrary to received theory, she suffered from a very large suppurating bubo on the same side as the sore, which had not the slightest induration, it secreted pus freely, was painful, and extensive, having all the characters attributed to the soft non-infecting sore, but when tested auto-inoculation failed.

SERIES No. 4.—*Primary Sore—Non-indurated—Roseolar and Papular Rash—No Inguinal Enlargements—Inoculation from a Vaginal Discharge—Formation of Characteristic Pustule—and Soft Sore—Cure of the Constitutional Signs.*

No. 1,075, a remarkably strong healthy-looking girl of 21, who had been a domestic servant till within two months, was admitted April 27, 1870. Ward No. 2.

About one week previously she felt a sore at the fourchette. On examination, this is ragged, pus-secreting, and painful; there are no inguinal enlargements whatever, and there is some slight discharge; but the rent at the fourchette is too sore to allow the use of the speculum.

On May 26th, being thirty-six days since first observing the sore, roseolar blotches appeared on the body and face, and in a few days more some scattered papules also were seen; the throat became patchy and painful.

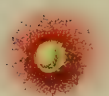
On May 31st, I inoculated her with her own vaginal discharge, using the speculum to see that the vagina was free from any sore. The result was negative, but some redness and anger appeared at the site of the inoculations for the first two days.

On June 7th, I inoculated from the artificially produced sore of No. 988, Series 3, but with negative result, as before detailed.

On June 10th, I inoculated again from the vaginal discharge of another

SERIES

4

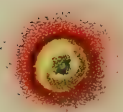


CASE

1,075

SERIES

5

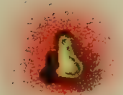


CASE

1,112

SERIES

6



CASE

1,060

OLDHAM, *fecit.*

SERIES 4.—Pustule produced from a vaginal discharge, as seen 24 hours after inoculation.

SERIES 5.—Pustule produced from a sore which was derived from another sore inoculated from the patient's own vaginal discharge.

SERIES 6.—Pustule produced by inoculation with the patient's own vaginal discharge.

patient, No. 1,098, suffering from marked syphilitic signs, but also with negative result.

On June 12th, I inoculated from the artificially propagated chancroid of the girl aged eight, forming the subject of Series No. 2, but with a negative result.

Being now pretty certain that there was some accidental miss about these inoculations, on June 13, I inoculated her from Case No. 1,093, a strong very dark skinned girl, having no sore, but suffering from copious mucous patches, discharge, papular rash, and pains, and having had, with the primary, the two months previously, a suppurating bubo; the extensive scar of which is very visible. This inoculation was a perfect success, the pustular stage, after twenty-four hours, is well shown by the drawing, taken at the expiration of this time, a perfect chancroid, as large as 6d. formed. The patient rapidly improved from the date of re-inoculation, and the rash completely disappeared. I did not think it necessary to test the inoculability of this artificial sore further, as the appearance was so perfectly marked.

On July 7th, I inoculated this patient from the vaginal discharge of Series 12, forming a perfect pustule and sore.

This case shows that failures may occasionally and accidentally occur thus: the virus of Nos. 988 and 1,118 failed at the first trial, while that of 1,093 had failed with other patients, though it succeeded admirably in this instance.

SERIES No. 5.—Primary Sore of the Right Labium—Non-indurated, but presenting the general appearances of an infecting Sore—Tender and Indurated Inguinal Glands on the same side—Copious Papular Rash evolving itself—Primary Sore resisting Inoculation—Inoculation from a Vaginal Discharge.

Patient aged twenty-two, No. 1,112, married, has a healthy child living; her husband died of phthisis two years since; four months ago went on town, and first felt a sore one week before admission, on May 13, 1870. She is now in tolerably good general condition, and has a sore about the size of a fourpenny piece on the labium, with non-indurated base, but having a clean surface, secreting but little pus, and resembling the general appearance of an infecting sore, minus induration, or even a parchment thickening; a slight enlargement could already be felt in the inguinal region, and the glands became rather dense.

May 16th.—I inoculated her on the side from this sore when unirritated, but without success.

May 23rd.—Some murkiness and rash is appearing about the face.

May 31st.—I Inoculated with gonorrheal matter from a male patient, but without success.

June 3rd.—I Inoculated from her own vaginal secretion, which is hardly a pus secretion, with negative result.

June 7th.—I Inoculated with the vaginal muco-pus of a girl aged seventeen, No. 1,140—suffering from patches of the vulva, with purpuric, and lichenous spots—with this I produced a characteristic, but not vigorous pustule, which faded and dried up by the 12th of June.

A copious papular rash had now showed itself on the face and limbs.

June 12th.—I now inoculated from the second artificially produced pustule of 988, Series 3, being the grandchild, so to speak, of a vaginal discharge, and it produced, by the 16th, the well-marked pustule shown in the illustration—the angry base—depressed centre and peculiar pustule is very typical indeed.

On June 13th, I inoculated the other side from the vaginal discharge of Case, No. 1,093, but with a negative result; yet on the same day, and from the same patient, I also inoculated case the represented in Series 4, with a perfect result; showing, I think, some informality in this inoculation.

On June 16th, I performed a most interesting experiment. A patient, No. 911, who had been for over three months in the laundry of the Institution, and who had been nearly two months under my care, in the hospital, previously (so that she was under observation and absolute restriction for more than five months), and had no sore for over four months, came under my care again, with papular rash, alopecia, and patchy ulceration of the throat. I made a vaginal examination and found a muco-purulent discharge still existing; with this I inoculated, the subject of this series, on the opposite side of the abdomen, and produced a most characteristic pustule and well-marked sore.

On June 27th, I inoculated the patient from this vaginal discharge of the patient, forming Series 11, and produced a most characteristic pustule and chancroid.

There is no doubt but that this pustular sore, of June 16th, the offspring of the vaginal discharge, of an intensely syphilitic woman, in another intensely syphilitic, was indefinitely transmissible, and that also the former inoculation of June 12th, as the 2nd product of the infected patient, series 3, grafted on this other equally intensely infected, was also indefinitely transmissible.

If either of these double distilled poisons were communicated to a healthy male, it might surely be predicated that though the appearance would be that of a soft sore, it would communicate the taint. Yet what is here demonstrated experimentally and artificially,

I have no doubt, does frequently occur, as I have often seen non-indurated sores and suppurating bubos, and inoculable sores, followed by constitutional signs in the female, and not very rarely in the male.

SERIES NO. 6.—*Inflammation of the Labia—Condylomata—Papular Rash—Vaginal Discharge—Disappearance of the Rash under Inoculation—Treatment.*

No. 1,060.—A strong girl, aged nineteen, about twelve months on the town, and never before diseased; was admitted April 22, 1870—Ward 2.

She had observed, one month before, a sore at the labium, which was not very painful. She has now inflamed and tumid labia, patchy sores, and elevated condylomata, and a papular rash.

There was a large surface of elevated pale patchy sores between the labium and the thigh, and, on vaginal examination with the speculum, no sore or abrasion could be detected, but there was an appreciable amount of muco-purulent discharge.

On May 30, a sore was observed on the mons veneris opposite to one of the ulcerated mucous patches on the thigh, having much the appearance of a soft sore; from it I inoculated the abdomen, and on June 3, a small pustule formed; but by June 7 had died away.

June 7.—I inoculated the other side of the abdomen from her own vaginal secretion, which was then not very abundant; this formed a well-marked pustule with depressed centre and somewhat inflamed base—the drawing of which was taken on the 14th June—seven days after the inoculation. The next day this burst and gradually dried up, the small ulcer, left by it, healing up by the 20th. The entire stage of this sluggish, but not abortive inoculation, being thirteen days.

June 16.—I inoculated with the vaginal discharge of No. 1,093, being the same day that I inoculated so successfully, the pustule represented in the drawing, Series 4, from the same source, but produced no pustule.

June 17.—I coated the almost dry inoculation of June 7 with the vaginal muco-pus of 1,024, Series 1, without any effect.

June 25.—I inoculated in two places the abdomen with vaginal secretion, now hardly purulent, of 988, Series 3, now three months since her admission, and produced most characteristic pustules and sores, equalling a shilling in size, which are well represented in the illustration, after they had partially coalesced.

The rash had quite disappeared by July 6; the mucous patches and labial tumefaction still partially existed.

SERIES NO. 7.—*Inoculation from a Sore, the product of Vaginal Discharge—Disappearance of the Rash.*

No. 1,159, was admitted June 4, 1870, aged 26, rather broken down in health, and the mother of a remarkably healthy child, born before her first infection in June, 1868. On that occasion she had a well-marked ulcer on the uterus and on the back part of the vagina, followed by most intense papular rash and cachexia.

The stains of the rash are still seen, and there is a slight but marked fresh outbreak. She has a little vaginal discharge, and on speculum examination there is no sore or abrasion to be seen.

June 8th, I inoculated, from her own vaginal secretion, unsuccessfully.

June 9th, inoculated from vaginal discharge of 988, without success.

June 12th, I inoculated, from the artificially-produced sore of the girl No. 1,118, Series 2, aged 8, which had been artificially produced from a vaginal discharge, Series 1. This produced a most characteristic pustule and sore, with sharp cut edge pouring out abundant pus, and no doubt indefinitely communicable.

June 20, inoculated, with a pin, from her own vaginal discharge, but without result, a small ulceration simply following.

The inoculation of June 12 was then a perfect chancroid. Her condition became much improved, and the rash disappeared.

SERIES NO. 8.—*Inoculation from a Sore of the Second Descent from the Vaginal Secretion of a Patient while suffering intense Syphilitic Intoxication (Series 3), on another Patient intensely Syphilitic—Production of Characteristic Pustule and Soft Sharp-edged Sore, Secreting abundant Pus—Remarkable Improvement of the Symptoms.*

Patient, aged 28, No. 1,163, admitted June 7, 1870; broken down and thin, having been delivered of a dead child two months before. Is now covered over with papular rash, and suffers severely from pains.

June 7—Inoculated, from her own vaginal secretion, which was hardly purulent, without result.

June 10—Inoculated from the pustule of 988, Series 3, the second generation of the vaginal discharge on her own person, producing a characteristic small pustule, with non-depressed centre, having a well-marked sharp-edged ulcer, I have no doubt indefinitely communicable, and in every way resembling a contagious sore.

The alleviation in the symptoms of the patient since the formation of the artificial sore is most remarkable. The rash has immensely improved. She is, however, phthisical, and left hospital free from signs, July 15.

SERIES NO. 9.—*Primary Sore—Double Suppurating Bubos—Pains—Copious Papulo-squamous Rash—Intense Cachexia and Debility—Inoculation from a Sore the Artificial Product of a Vaginal Discharge—Inoculation from the Patient's Vaginal Discharge producing a Sore on another Patient—Relief of the Symptom.*

Case No. 1,000, admitted March 26, 1870, aged 19. Left home ten months ago, and has been since on the town. Only two days before admission she observed a sore on the labium. At this date it was large, painful, pus-secreting, and without the slightest induration. There was a tender gland in the left groin.

This gland was leeches, &c., but failed to be resolved, and was opened on April 25th, giving exit to a large quantity of pus. I inoculated with this on the side, producing only an abortive pustule.

On April 30 I made a speculum examination, the parts having been before this too sore to allow it. There was not the least ulceration of the vagina or uterus, but some muco-purulent discharge.

April 30—I inoculated from the second artificially-produced pustule of a male patient, the second generation from a sore on the prepuce, but with negative result although the pustule was very vigorous.

May 2—A copious rash began to make its appearance over the entire body, and she became profoundly cachectic—in fact, a specimen of syphilitic taint.

At this date I inoculated from her own sore with negative result. The patient continued in a very prostrate and cachectic condition; tonics, stimulants, good diet, iodide of potassium, iron, and creasote being used.

On June 3 the patient had decidedly improved in general health, but another large suppurating bubo was forming in the opposite groin. I opened this, giving exit to a large quantity of thick healthy-looking pus. I also inoculated the abdomen the same day with this pus, but with a negative result, her condition being now very cachectic.

June 8—I inoculated with her own vaginal muco-purulent discharge, but failed, though there was a slight attempt at a pustule.

June 10—I inoculated from No. 1,118, Series 2. This produced a pustule and specific ulcer by the 13th, which became a regular chancre, equalling a 6d. piece in size.

June 12—I inoculated the opposite side from the same source, No. 1,118, Series 2, producing also a specific sore.

On July 7 I inoculated from case forming Series 12, producing a perfectly marked pustule and sore.

On June 15 I inoculated No. 1,098, a strong woman, covered with papular rash, from her vaginal discharge, and produced a tolerable but not well-marked pustule. (This same patient, 1,098, was specially difficult

of inoculation from other sources.) From this date the symptoms steadily improved, the use of tonics and good diet being alone superadded.

It is remarkable that I failed, on April 30, to produce a pustule on this patient from an inoculated sore on a male patient, which I produced artificially from a preputial sore; yet on June 10 and June 12 I succeeded in doing so from the case 1,118, Series 2, whose sore was the descendant of a vaginal discharge.

SERIES NO. 10.—Primary Sore—No Inguinal Enlargements—Inoculation from the Artificially-produced Sore of Case 1,118, Series 2.

Patient 1,085; married; aged 21; admitted May 2, 1870; W. 2. Never had children. Her husband left her, and she cohabited with another for about one week. She has now a well-marked, small, clean-looking sore at the upper part of the vulva, above the clitoris. There is no induration at the base; no inguinal enlargements or tenderness. The speculum shows no sore or abrasion, and the uterus is healthy.

May 18—I inoculated her from the sore, producing a most characteristic pustule and sore.

June 3—I inoculated the side, from vaginal muco-pus, unsuccessfully.

June 7—I inoculated the side again, from the vaginal muco-pus, unsuccessfully.

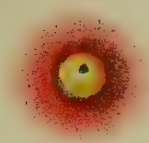
June 12—She has been troubled with flying nocturnal pains and malaise. I inoculated to-day from the artificial sore of 1,118, Series 2, the product of the vaginal discharge of 1,024, Series 1. This produced a characteristic pustule and sore, the exact resemblance of the inoculated chancroid from her own primary sore, but it was more marked and persistent.

SERIES NO. 11.—Auto-Inoculation from Vaginal Discharge—Transmission of the Auto-Inoculation to other Patients—Inoculation from the Vaginal Discharge almost without a Failure—No Rash, Pains, or Marked Constitutional Signs.

A girl, aged 20, No. 1,185, was admitted June 1, 1870, and gave the following history, which has been several times tested, but always with the same result as to accuracy and description:—

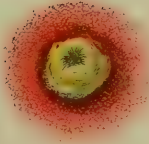
In August last she cohabited with one person belonging to a regiment, with which he left 1st September. About the first week of September she felt irritated and sore, and got a discharge, but was never aware of a sore beyond irritation. This discharge continued ever since, very profusely, now about ten months. She states positively that she never cohabited since then with any one, and that about two weeks before admission the labia became swollen while she was hard worked as a servant. There is now considerable tumefaction of the labia, a few mucous patches are about the posterior part of the vulva, and there is a considerable thin purulent discharge. No rash, pains, &c., but she is pallid

SERIES
11



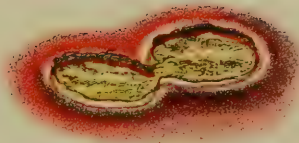
CASE
1,185

SERIES
12



CASE
1,220

SERIES
6



CASE
1,060

OLDHAM, fecit.

SERIES 11 — Pustule produced by inoculation from the Patient's own vaginal discharge (three days old).
SERIES 12.—Pustule produced by inoculation from the patient's own vaginal discharge (four days old.)
SERIES 6.—Chancroids resulting from the coalescence of two inoculations from a vaginal discharge.

and cachectic. Assuming the history true (which I had reason to believe it was, and that the chief annoyance had been the vaginal discharge), I carefully tested its capability of producing chancroids or pustules as follows :

On June 27, I inoculated this patient on the abdomen, from her own vaginal discharge, producing a most well-marked pustule by the 30th.

On June 27, inoculated No. 669 unsuccessfully.

On June 27, I inoculated No. 911 most successfully; now (July 4) a regular chancroid; pus secreting and soft.

On June 27, inoculated No. 1,169 most successfully.

On June 27, inoculated No. 1,112, producing a pustule on the third day, and now (July 6) a regular chancroid.

On June 27, inoculated No. 1,098, producing a pustule on the fourth day, which became a marked chancroid.

On June 29, inoculated No. 1,098, producing a pustule on the third day, and became a chancroid almost as soon as that of 27th June.

On June 30, inoculated No. 1,093, producing a pustule in twenty-four hours, which became a most vigorous chancroid.

Only two failed of all the inoculations I tried from this case.

June 29—I inoculated, from the pustule produced on the patient's own person, on a patient No. 1,167, who had resisted hitherto several attempts at inoculation from other sources, and produced a regularly formed and acute pustule in twenty-four hours; now a chancroid (July 6).

June 29—Inoculated 1,138 from the vaginal discharge, producing a pustule and sore by the 7th July.

July 4—Inoculated from her own mucous patch, unsuccessfully.

June 29—Inoculated 921, from the vaginal discharge, forming a pustule which dried up by July 6.

June 30—Inoculated 1,189, forming a pustule, and marked sore.

Some of these patients had inoculations on them at the time, as Nos. 1,112 and 1,093.

Nothing could be more potently inoculable than this discharge; the figure of the resulting pustules was most characteristic. The patient still (July 6) has no signs beyond the mucous patches. On the other hand, I have inoculated several patients from a case in hospital, suffering from vaginitis and discharge, but never succeeded in producing a pustule or even tolerable irritation.

SERIES NO. 12.—*Vaginal Discharge only—The Primary Sore—No Marks—Constitutional Signs—Inoculation from Vaginal Discharge.*

No. 1,220.—An uncommonly fine healthy-looking girl of twenty-three, was admitted July 6, 1870. She was in hospital during the last two years, on two occasions, suffering as follows:—

On June 1, 1868,—From several soft sores.

On October, 1868,—From a sloughing phagedenic sore of the nympha, followed by patches, papules, pains, and finally by ecthymatous spots here and there over the body.

The phagedenic ulceration was most acute, and was cured by the use of escharotic and local applications.

She remained without further symptoms till about ten days before admission, when she got a discharge with some pain and tenderness, and a sore formed, apparently secondary in character, at the cicatrice of the former sore near the nates; the stains left by the ecthymas are still very evident; also a wound on the arm, made about six weeks ago, presents a deep coppery stain, indicative of a latent taint.

On careful examination with the speculum, no ulcer whatever is to be seen, but there is a thick purulent vaginal discharge.

I inoculated with this on herself, on July 7, producing, by the 11th, a well-marked pustule, becoming a sore shortly afterwards.

July 7, I inoculated No. 1,193, suffering from copious patches, alopecia, and pains, and produced, by the 11th, a characteristic pustule and sore.

On July 7, I inoculated No. 921 (who was at this time partially under the influence of another inoculation), and produced, by the 11th, a perfectly formed umbilicated pustule. This patient was intensely syphilitic at the time.

On July 7, also inoculated No. 988, who was almost free from syphilitic symptoms, having been frequently inoculated, as referred to at Series 3, but produced only a small ill-formed pustule and superficial sore.

July 7, also inoculated No. 1,000 (referred to at Series 9), who had already been several times inoculated, and produced a specific, but not vigorous pustule.

July 7, inoculated No. 1,075 (referred to at Series 4), and at the time suffering from a vigorous chancre; produced a perfect pustule and small chancre.

This case is very interesting, as there was no rash, and the secretion from the sore could not possibly be mingled with the vaginal discharge. The sore had not the character of a primary either in appearance or secretion, and the patient might otherwise have been looked on as a specimen of rude health, but as evidenced by the stains, and the discoloration left by the wound occurring within two months, together with the reopening of the original cicatrix, the venereal taint was, though lateal, still as active as ever.

Table of Remarkable Inoculations from Vaginal Discharge—all producing Characteristic Pustules and Chancroids more or less persistent, and capable of Indefinite Propagation.

Inoculation made from No. 1,024.

June 2nd, produced chancroid on No. 1,118;

which produced chancroids, - June 10, on No. 1,000.

June 12, on No. 1,000.

Do. on No. 1,085.

Do. on No. 1,159.

No. 988.

May 23, do. on herself. From this, June 3, on herself.

Do. „ 10, on 1,163.

Do. „ 12, on 1,112.

June 3, do. on herself.

„ 13, do. on herself.

„ 20, do. on 1,060.

„ 23, do. on 921; not very developed.

„ 23, do. on herself.

No. 1,185.

June 27, do. on herself. From this, June 29, on 1,167.

„ 27, do. on 921.

„ 27, do. on 911.

„ 27, do. on 1,169.

„ 30, do. on 1,189.

No. 1,140.

June 7, do. on herself.

„ 7, do. on 1,112.

„ 15, do. on 1,138.

No. 1,093.

June 13, do. on 1,075.

„ 23, do. on 1,189.

No. 1,060.

June 7, do. on herself; not very vigorous.

No. 911.

June 16, do. on 1,140; not very vigorous.

No. 1,220.

July 6, do. on herself.

„ 7, do. on 1,193.

„ 7, do. on 921.

„ 7, do. on 1,000; not very vigorous.

„ 7, do. on 988; not very vigorous.

„ 7, do. on 2,075; not very vigorous.

These twelve cases I have selected from many others under treatment, and where the inoculations have been made so as to test the question of the source of the soft-chancroid, or usual sore.

It seems to be essential that the system shall be under the influence of the constitutional poison in order that the vaginal discharge may be capable of producing the characteristic pustule and sore. My inoculations, I think, tend to this conclusion; but I hesitate as yet to pronounce it as an invariable rule.

I have inoculated from various discharges where the syphilitic taint appeared absent, but have not succeeded. I have inoculated from the mucous patches at the anus of a child two years old, with inherited syphilis, and produced a characteristic inoculation. While in some of the cases detailed, as well as in others, I have inoculated from the mucous patches, but did not succeed. I have also failed in inoculating from large soft pus secreting sores, but succeeded in the same instance with the vaginal discharge.

If the mucous patch can be the progenitor of the soft sore, as proved in the case of the child, and that mentioned by Mr. Richardson in the notes he has given me, a vaginal discharge, in order to be inoculable and productive of the soft sore, seems necessarily to be co-existent with constitutional symptoms, latent though they may be, it may be reasonably concluded, though contrary to the theory entertained by many authorities, especially on the Continent, that the chancroid or soft-sore is not the result of contact with a special virus, or derived from an untainted system, but rather developed from the secretions of an individual who is thoroughly under the syphilitic influence; and by its very formation indicates this malign source.

In the series of cases tested and treated by the inoculation method, the result has been most favourable in most instances, especially in those who were most prostrated by the disease, and thoroughly cachectic. No matter what our prejudices may be, or the difficulty that we may meet in explaining satisfactorily the mode of cure, we should not, I think, neglect any proposition based on practical results, which may enable us to combat the varying phases, and subtleties of the disease with greater prospect of success. The progress made towards recovery in cases of great intensity as in those forming Series 3, 8, 9, was most striking, and was even more remarkable in a case of acute rupia, lately under my care (occurring as the first eruption after a primary sore) in a girl of twenty, who showed the most striking improvement when inoculation had been

performed; two inoculations failed, the third succeeded; the rupious crusts fell off and healed most satisfactorily within one fortnight. Every one conversant with the subject of constitutional syphilis has seen the inexplicable freaks and spontaneous cures which occur; but where after inoculation we see almost every case undergo a marked improvement, it is impossible to attribute this to some accidental or unknown cause.

The patients themselves (who are not very ready to undergo the slightest approach to an operation) from seeing the improvement in others, willingly submit to this treatment.

I do not repeat the inoculation till the previous one has healed, and sometimes this lasts three weeks or one month, and it is a remarkable physiological fact, that the more infected and debilitated the patient, as in Series 3 and 9, the greater is the resistance to the inoculation, and, when produced, the greater is the tendency to heal; while in the more vigorous and less depressed cases, as in Series 2, the contrary holds good.

Professor Boeck, the great advocate of the inoculation treatment, urges that the inoculation should be made from a previously irritated hard sore; but the source from which I obtain the infection is, I think, just as efficient, if not more so, than the sore itself would be—for long after the primary sore has healed, and when the patient is fully influenced by the constitutional ferment, I can produce these pustules, while on the other hand I have not as yet been able to produce them where the history of a previous infection, even though several months antecedent, was absent.

It is noteworthy that failures in these inoculations take place when practised, as by Professor Boeck, from the sore itself, just as they do from the vaginal discharge. I believe in the latter instance, owing to the large admixture of the vaginal mucus, as shown by microscopic examination, and is also remarkable that while inoculations from a hard sore produce pustules which are more or less evanescent and require a succession of the operation; inoculations from the vaginal discharge produce a regular pustule which, in proportion to the cachectic condition of the patient, is more or less vigorous and enduring.

No doubt this treatment is slow, yet it can be carried on without much inconvenience to the patient, and does not prevent the use of tonic and other remedies. In this disease, so eccentric in its manifestations, we should not despise any method, but suit our treatment carefully to each case, just as with mercury, which we see, for example,

acts so incomparably well in infantile cases, yet would be destructive in many other manifestations of the same syphilitic poison.

I do not bind myself to any special rigid rule of treatment. Some cases I treat mercurially, some by the inoculation, and some by the sweating method; all have their recommendations and adaptibility to certain cases; but where constitutional cachexia exists, I think the inoculation has, on the whole, been most satisfactory, though it is more troublesome to the surgeon, and, perhaps, tedious to the patient. While on the other hand, it cannot "go wrong," as mercury, though most carefully administered, occasionally appears to do. Mr. Richardson has kindly furnished me with the appended notes of cases treated by inoculation, at the Adelaide Hospital, which confirm the remarks I have made as to its use, and testify to its utility.

CONSTITUTIONAL SIGNS FOLLOWING INOCULABLE SORES.

I have now given the results of some inoculations performed from female secretions, where, undoubtedly, a syphilitic taint existed in both recipient and originator; but in addition I have also frequently inoculated patients in the hospital from their own primary sores, yet constitutional signs frequently ensued. In a previous clinical review of cases under treatment at the hospital, given in this Journal, November, 1869, I stated, what I still repeat, as the result of direct observation of the cases under my treatment, excluding from view theories, or the continental experiences, so often adopted in this country, as to the exact division of infecting and non-infecting sores, that, as a rule, I observed all the sores—whether soft or hard—were followed by constitutional signs. In the last number of this Journal exception is taken by an ardent "dualist,"^a in his paper on "The Treatment of Syphilis," to this statement, and sooner than admit this, the more convenient explanation is suggested—"that the women might have two kinds of venereal sores at the same time."

In that clinical review of cases I stated that of "54 cases admitted not previously diseased, but 4 had escaped constitutional infection." The patients themselves were mostly under 20 years of age, and many were only a few months from their home in the country. These were never before on the hospital books—had no signs of any previous disease—and, of course, they were regularly

^a Dr. Barton, F.R.C.S.I., &c. *Dub. Quart. Jour.*, May, 1870.—Some Practical Remarks on the Treatment of Syphilitic Disease.

examined, as in all such hospitals, with the speculum; but no sores could be detected. These patients had no object whatever in misstatement; on the contrary, they are generally anxious to give the fullest account of their affection, as these younger girls are much alarmed at being diseased. It is quite a mistake to suppose they wish to mislead—unlike cases in private, in young men, or the married—where there may be such a temptation; there is but little here. Of 22 of these cases selected as most certainly not previously diseased, I produced auto-inoculation successfully on 11, and 11 failed; but not one of the former failed to show constitutional signs, while only one of the latter seemed to escape at that date. The non-use of the speculum is always the favourite resort of those who uphold the dual theory of sores, but at the hospital here, both myself and my colleague, Dr. M'Dowell, very rarely indeed find a sore absolutely inside the vaginal canal. (I have the drawing of one on the back of the vaginal wall and on the uterus, which is the only true, well-marked case I have seen during two years.) Ulcers of the uterus are, no doubt, occasionally seen—associated sometimes with constitutional signs following an external sore. Both myself and my colleague have tried auto-inoculation with these, and I have tried inoculation from them on a second subject, but without result. I rather think they are simple non-specific ulcers.

The connexion between the appearance of the constitutional signs and the existence of the primary sore in the cases alluded to, was, I may say, invariable; generally before leaving the hospital, and often before the healing of the sore, they had commenced to appear. The writer, in commenting on my cases, suggests—firstly, that “these soft sores had nothing to say to the syphilitic symptoms;” but that there had existed a truly infecting sore, probably unnoticed. This is exchanging the practical for the theoretical. We do see in these 54 cases soft sores; and 50 of these produce infection signs. Yet it is proposed to introduce another sore not seen, and by no means likely to have existed—for a truly hard sore in women is very persistent, and, therefore, not at all apt to be overlooked as suggested. “An equally satisfactory and probable” explanation is then suggested—that these soft sores were “true syphilitic chancres, in which the sign of induration was wanting.” This is a very delicate “locus penitentiæ” for those who hold to the uniform character of the infecting sore—as thus one of its chief indices is given up, its next character on which stress is laid—*i.e.*, the condition of the enlarged and dense inguinal pleiad of glands, in women is by no means

marked, and thus its distinctive characters would be lost. But while endeavouring to support preconceived ideas, by supposing the existence of a sore which could not be found to exist, or that the soft sore was but an infecting sore, or hard sore—minus hardness—no explanation is attempted as to the inoculability of the sores, as to the occurrence of suppurating or inflamed bubo, or as to the appearance of the sore being the “soft sore,” or “number of soft sores”—a term so well known, that I am sure it is hardly necessary to describe more in detail for the readers of the *Dublin Quarterly Journal*, and which it will, I think, be allowed, as I see many hundreds yearly, I could without difficulty recognize; and I do still form, as the writer remarks, the “strange conclusion” that the secondary syphilitic symptoms followed from them.

It is matter of daily observation at the Westmoreland Lock Hospital that infection signs follow sooner or later almost every case of primary sore; but the series of cases I have detailed, when alluding to inoculable vaginal secretions, will prove abundantly that the usually believed infecting type of sore, or glandular enlargements, is a rarity in these women, who must be accepted as the true specimens of the disease in Dublin.

I have received the most recent information on this question from the Government Lock Hospital, Aldershot. June 9, 1870, the surgeon states:—

“Undoubtedly many patients are admitted suffering from general syphilis, in whom soft sores only have been detected, with or without suppurating bubo, even while in hospital with inoculating sores only, secondary manifestations do occasionally occur.”

From the Government Lock Hospital, Cork, out of 324 cases the surgeon states:—

“Amongst the sores I have not met with more than two or three which could be considered in any way indurated—almost all were superficial—secreting pus or muco-pus, without any marginal thickening, elevation, or induration; in fact, they all had the character of the soft sore. The indurated chancre in the female *being very rarely met, with here.*

“Of this I am positive that I have had here several cases of what is usually known as the true Hunterian chancre, under my care in the male; and, as I have stated before, I have not had one well-marked case of the same form of chancre in the female. My experience is that the indurated chancre is not invariably followed by

rash, sore throat, &c.; it is usually so followed, but not as frequently as generally supposed."

It is remarkable that the description of the primary sores at the Lock Hospital, Cork, and here, agree so exactly—yet in both places they must be considered as, at least, the chief sources of the disease as communicated to the male population.

From the Lock Hospital, London, July 4, the surgeon states:—

"Severe secondary symptoms have also frequently occurred after soft sores. A considerable number of soft sores are also followed by symptoms; there is not much difference in their severity, whether following a hard or a soft sore."

It is evident that in the class that constitutes the contagious source of the disease in London, Aldershot, and Cork—from whence I have obtained information. It is of little importance as bearing on the future of the case, whether the primary sore be a hard, or infecting so-called, or soft non-infecting.

I am free to admit that "soft sores," with or without suppurating bubo, are found to occur as the only lesion in the male (who is usually young and vigorous at the time of its reception); and that constitutional signs often do not show themselves—general hospital and private practice in Dublin must satisfy every surgeon of this. But that there is such an established immunity from constitutional signs in these cases I cannot agree to. I believe much depends on the source from which the soft sore was derived, and also on the constitution or diathesis of the patient.

Several cases in the male, carefully noted, and under observation from their very commencement by a large class of students, prove the succession, in their usual order, of constitutional signs after soft sores tested by inoculation.

It is stated by syphilographers that suppurating bubos, or inoculable sores, are seldom, if ever, followed by constitutional evidences. That it is not so very uncommon here is shown by many examples, of which I select two:—

A healthy man, T. C., aged forty-eight, presented himself at Mercer's Hospital for treatment, a shoemaker, and the father of healthy children, seven of whom are alive, and none of whom or himself ever had any rash or other sign of disease. He contracted a sore in July last, which appeared within one week after connexion, and was situated on the inner side of the prepuce—soft, pus-secreting, about the size of a fourpenny piece, and without any glandular enlargements. I drew the attention of the class of students to this as a good specimen of the non-indurated sore

of the prepuce, and one suitable for testing the double virus theory. On the tenth day after the formation of the sore, I inoculated him on the thigh, and a most characteristic pustule formed, which went through its usual stages, of which he bears the cicatrix.

The primary healed by topical applications without any trouble in two weeks, and in four weeks afterwards a copious papular eruption appeared over the body; he got nocturnal pains, alopecia, and became cachectic; and finally with a fresh evolution of papular blotches, had severe iritis of the right eye, and had mucous patches at the mouth and anus occurring nine months after the inoculation and healing of the sore; on examining the penis at this date no cicatrix of any kind was visible.

The patient, an intelligent tradesman, avers most positively that he never was in the way of contracting disease since the occasion in July, 1869. A more marked case of constitutional infection could not be, the signs appearing in usual order from the occurrence of the sore.

J. K., a strong young man, aged twenty-two, a tailor, never before diseased. In July, 1869, contracted a soft sore, pus-secreting, extensive, and painful, on the inner side of the prepuce; this became inflamed and poured out much pus. I admitted the patient to hospital, and in a few days a lymphatic bubo formed at the root of the penis. I opened this; and with the pus I inoculated on his right side; a characteristic pustule formed, and from this one, on the 6th day, I produced another on the same side. A wax mould was taken of the parts when in this stage, which now is in the museum of the Royal College of Surgeons, and demonstrates the state of the penis, the absence of indurated glands, and the occurrence of the lymphatic bubo. In four weeks, and without leaving hospital, the evidences of constitutional infection appeared—alopecia, enlargements of the nuchal glands, and a copious fine papular eruption, and roseola covered the entire body. He was carefully treated without mercury, and with creasote. All cleared away by degrees, and now, nine months after the sore and inoculation, mucous patches affect the lip and tongue.

This patient states most positively (and he has no possible object in misleading) that he never had any other sore whatsoever, and there is no cicatrix or questionable history.

In contrast to these, the following is interesting as a case of indurated sore of two months' standing, which became *auto-inoculated* on the abdomen.

J. H., aged 40, presented himself at Mercer's Hospital, suffering from a sore on the dorsum of the penis, with a distinctly elevated and dense base, not pus-secreting and not painful. He is married,

the father of several grown-up healthy children; was never before diseased, and contracted this sore within two months. The glands in the groin are discernible to the touch, indurated, and forming a pleiad tolerably dense. He lived with his wife, and cohabited with her while having the sore.

Being a shoemaker, and sitting at his work constantly, the dorsum of the penis lay against the abdomen, and produced a well-marked inoculation. The auto-inoculated sore was distinctly indurated, on an elevated base, and identical in appearance with the sore on the penis. The glands in the groin were dense and tumid, and well-marked scattered papules appeared on the head and trunk. So interesting and characteristic of induration and inguinal enlargements were their appearances that a wax mould was taken of the parts by Mr. Lyddon, and now in the Pathological Museum, Royal College of Surgeons. He left hospital, before being cured, on July 28, 1869.

His wife, aged 38 to 40, came in, under my care, to the Lock Hospital married ward, in September, suffering from a large pus-secreting non-indurated sore, which was succeeded in due time by patches, pains, papular rash, murky skin, iritis, patches of the mouth, and cachexia.

The following married case represents the transmission of a sore, accompanied by suppurating bubo and intense constitutional infection in the husband, producing a large soft sore, without inguinal enlargement, or other signs in the wife:—

P. M., aged 23, married four years, previously healthy, was infected by a woman of the town at the latter end of February. He got a non-indurated sore at the frenum, and shortly a large suppurating bubo formed in the left groin. This was opened, and in about one month an eruption of scattered papules appeared over the body, and the bubo was healing. He now began to drink and got into poverty. Immediately the bubo became ulcerated, he became cachectic, and numerous severe ecchymatous, almost rupial, spots appeared, which are now (May 30, 1870) healing, but the sore is not yet quite well. Treated by creasote mixture and tonics.

This man's wife was admitted to the Lock Hospital on the 18th March, 1870, with a large non-indurated sore on the labium, communicated by him, and which he admits. There is no bubo or any glandular enlargement whatever. She is suckling her child, ten months old, a remarkably healthy one. The sore was much improved

in a few days, but she left the hospital uncured. I have heard that no further signs have appeared.

The most valuable "confrontations" that are seen at the hospital are between man and wife, and are much more reliable, I think, than those between dissolute women and men. The general evidence is, as in the preceding, that the sore on the female is non-indurated, pus-secreting, and, no doubt, inoculable, yet thoroughly infecting, no matter what may be the nature of the sore in the husband.

The subjoined table gives the result of the last fifty-four cases under my care who were diseased for the first time. It will be seen how small a proportion of these escaped constitutional signs, and it affords the best criterion as to the quality of the disease prevalent in this city, and of the immensely wide influence it must have on not only the present, but the future community.

Patients with Primary Sores, Diseased for the First Time.

Patients with Primary Sores, Diseased for the First Time—continued.

Nature of the Primary Sore.	Followed by				
	Papular Rash	Patches	Pains	Iritis	Roseola
One soft, not inoculated,	Yes	—	—	Yes	—
One large soft, do.,	Yes	—	—	—	—
Do., do.,	Yes	—	Yes	—	—
Soft sore, - - -	Yes	Yes	Yes	—	—
Hard sore, typically infecting,	Yes	—	Yes	Yes	—
Do., do.,	Scattered	—	—	—	—
Sloughing sore, - - -	—	—	—	—	—
Do., - - -	Yes	Yes	—	—	—
Chronic sore, - - -	—	—	—	—	—
Do., - - -	Papulo-squamous	—	—	—	—
Do., - - -	—	Yes	—	—	—
Do., - - -	—	—	—	—	—
Large soft sore at fourchette,	—	—	—	—	—
Do., do.,	—	—	—	—	—
Slight sore, do.,	—	Immense patchy vegetations	—	—	—
Ulcer of uterus,	—	—	—	—	—
Ulcer of uterus,	Yes	—	Yes	—	—

Produced a dead child.

Produced a living child.

All but suppurating bubo and sore throat.

With anal fissures.

Nothing observed during 33 days succeeding.

Not seen since.

No other signs.

Do.

Patients never before Diseased, admitted for the First Time; where the Primary was either healed or had not been recognized.

Mucous patches only	Papular	Patches	Pains	Iritis	Roseola	
3 cases	Yes	—	Yes	Yes	—	With discharge.
6 „	Yes	—	—	—	—	One produced a dead child ; all had discharge.
3 „	Yes	—	Yes	—	—	One with patches of the throat and with discharge.
2 „	—	—	Yes	—	—	With discharge.
3 „	Yes	—	Yes	—	—	With discharge.
2 „	—	—	—	—	—	With discharge.

22, ELY-PLACE, DUBLIN,
1st July, 1870.

MY DEAR MORGAN,—The following cases, in the subjects of which I had recourse to syphilization, are of interest. They confirm the view held by some surgeons that symptoms of syphilis diminish and sometimes disappear, while the patient is under this treatment, a great improvement being at the same time manifest in the general health.

Moreover, I succeeded, with one of the patients, in producing a characteristic soft sore from an ulcerated mucous tubercle, the patient being free from vaginal discharge, and her primary sore being healed.

Indeed, the first case is of peculiar interest also, the patient having been seized with variola shortly after the eleventh inoculation.

As soon as the skin had recovered from the effects of the variolous pustules, no trace of the syphilitic eruption was discernible.

Whether or not the apparent cure in this case is to be attributed to nature, or to the combined influence of the syphilitic inoculations and variolous suppuration, I shall not presume to say. At all events she continued free from any visible symptom of syphilis a couple of years after she had been submitted to syphilization.

Yours, faithfully,

B. WILLS RICHARDSON.

JOHN MORGAN, Esq.

Taken from the Case-books of Messrs. Maxwell Reilly and Samuel Knaggs, two of our most attentive pupils:—

Syphilization—Successful Inoculation with matter taken from an ulcerated mucous tubercle situated at the anus, the suppurating sore resulting from said inoculation having been the first-parent of several suppurating sores, the successful produce of Inoculation—Variola—Apparent cure of the Syphilis.

C. T., aged nineteen years, was admitted to the Adelaide Hospital, Dublin, in the commencement of January, 1865, but she had been in the Hospital the previous August, with both an eruption and with sores at the vaginal orifice. Mr. Richardson did not see her on that occasion.

When re-admitted in January, she had a suppurating circular sore on the right nympha, and the greater part of her body was covered with red copper-coloured blotches.

January 17th, 1865.—Mr. Richardson inoculated the right thigh with some matter taken from the sore on the nympha. This inoculation succeeded, and resulted in an excavated, circular, slightly indurated sore, about the size of a fourpenny piece. It was covered with yellow pus.

January 20th, 1865.—Some of this pus was inoculated by Mr. Richardson a little above No. 1 inoculation. The next day the inoculated part was surrounded with a red areola, about a quarter of an inch in diameter. On the 23rd of January pustulation had commenced in its centre, but which through accident had been rubbed and broken. Mr. Richardson made a third inoculation, a little above No. 2, with some of the pus afforded by the latter.

January 24th.—No. 1 inoculation has repustulated, but its areola has faded. The No. 2 inoculation has scarcely any areola.

January 25th.—No. 2 inoculation has repustulated. No. 1 inoculation pustule loaded with pus. Its areola is more faded. Pustule of No. 2 inoculation distended with pus.

January 30th.—No. 1 inoculation sore has not increased in size. Its margin is very sharp, and it is granulating. No. 2 inoculation pustule continues unbroken.

A bubo, having a dusky red integumental covering, has formed just above the right labium. Deep fluctuation can be felt in its centre.

February 1st.—No. 1 inoculation sore smaller. No. 2 inoculation is the seat of a sore identical in character with those in No. 1.

February 6th.—The sore of nympha has healed without local applications. Mr. Richardson made a third inoculation to the right of No. 2, with matter taken from the ulcerated surface of a mucous tubercle situated just in front of the anus.

A sulphate of copper and opium lotion was afterwards applied to the anal tubercles.

February 7th.—No. 3 inoculation (made with matter from mucous tubercle) slightly inflamed and pustulating. Her bowels being confined, to have some sulphur electuary nightly.

February 8th.—The bubo has opened spontaneously.

February 11th.—A well-marked pustule has formed at No. 3 inoculation. It is full of pus, and is surrounded by a bright red areola, having a badly-defined margin.

February 12th.—Pustule of No. 3 inoculation has burst and is scabbing.

February 16th.—The scab has separated from No. 3 inoculation, which is the seat of an excavated purulent sore, about the size of a fourpenny piece. It has a sharp, defined margin, and is very slightly indurated.

February 24th.—Nos. 1 and 2 sores healing, the apparent result of the copper and opium lotion. The sore of No. 3 inoculation is the size of a shilling, is excavated, its deepest part being beset with very small granulations. It is slightly indurated, and is surrounded by a narrow red areola. Mr. Richardson made a fourth inoculation, about three inches distance from it, with matter taken from this sore (No. 3). The latter being inclined to spread, he touched it with a 20 grain solution of nitrate of silver, and ordered the lotion of sulphate of copper and opium to be kept constantly applied to it.

February 27th.—No. 4 inoculation, while in the process of pustulation, was accidentally rubbed, and the pustule broken. No. 1 sore is healed; No. 3, healing.

March 14th.—No. 3 sore is now reduced to the size of a fourpenny piece. It is circular, excavated, and has a sharply defined margin. No. 4 sore scabbing. Mr. Richardson made a fifth inoculation outside of the thigh, about two inches below No. 4, with matter taken from under its scab.

March 17th.—No. 5 inoculation inflamed and apparently pustulating.

March 21st.—No 5 inoculation is the seat of a well formed pustule. Patient had some fever yesterday, the pulse having risen to 110, the tonsils being ulcerated and the tongue furred. To-day the pulse has fallen to 104; the tongue is cleaner, and the throat not so sore. Herpes has appeared upon the lips. The red copper-coloured blotches, although still decided, have faded.

She was given a diaphoretic mixture, and the tonsilitic ulcers were touched with muriatic acid in treacle. The ulcerated condylomata at anus are much reduced in size. They have been treated with nit. argenti and with the copper lotion. She has some soreness in the tendon of the right biceps (arm) muscle.

March 22nd.—Pustule of No. 5 inoculation larger, and full of pus.

March 28th.—The site of No. 5 inoculation is occupied by a scabbing sore identical with those already described. No. 4 sore scabbing.

March 29th.—No. 5 sore has ceased to scab, and is now open, and has a slightly elevated and sharp margin, the base being indurated and the surface purulent. Mr. Richardson made a sixth inoculation, a little above No. 4 sore, with matter taken from No. 5. Sores 4 and 5 dressed with copper lotion, and ordered to be occasionally touched with a 20 grain solution of nitrate of silver.

April 1st.—No. 6 inoculation pustulated.

April 7th.—No. 6 inoculation is now the seat of a well-marked suppurating sore, about the size of a threepenny piece, having a suppurating surface, a sharply defined margin, and a red areola. Its base is slightly indurated.

April 15th.—Mr. Richardson made a seventh inoculation two inches higher up the limb than No. 6. No. 6 to be dressed with copper lotion.

April 17th.—No. 7 inoculation pustulated, and surrounded by a red areola.

April 19th.—No. 7 inoculation the seat of a circular sore a quarter of an inch in diameter. It is purulent, but not so excavated as any of its predecessors. No. 6 sore healing. Bubo cicatrized. The copper-coloured blotches are about as decided in colour as they were on the 21st of March. She has gained in weight since her admission to hospital. No. 7 inoculation is more excavated, is a little increased in size, and its margin has become better defined.

April 27th.—No. 7 inoculation still further excavated, and filled with tenacious pus. I made an eighth inoculation, with pus from No. 7 sore, outside and below No. 6 sore. No. 7 sore to be dressed with copper lotion.

April 28th.—No. 8 inoculation pustulating.

April 29th.—A well-formed pustule, with a red areola, on site of No. 8 inoculation.

May 1st.—Pustule of No. 8 inoculation has given way, exposing a circular, purulent, but slightly excavated sore, about a quarter of an inch in diameter. Its areola is very narrow.

May 6th.—No. 8 sore is sluggish, and has a tendency to scab. Mr. Richardson made a ninth inoculation two inches higher than No. 8. The copper lotion to the latter.

May 8th.—No. 9 inoculation inflamed, having a broad areola. No. 8 sore scabbing. No. 7 sore granulating, its margin being elevated and rounded off.

May 9th.—No. 9 inoculation pustulated.

May 13th.—A circular, purulent sore, about a quarter of an inch in diameter, on the site of No. 9 inoculation. Its margin is raised and defined, and it is surrounded by an areola about half an inch wide. No. 8

sore has scabbed. Mr. Richardson made a tenth inoculation, with pus taken from No. 9 sore, two inches higher than the latter.

May 15th.—No. 10 inoculation has pustulated.

May 18th.—A circular, slightly excavated purulent sore, having a defined margin, and narrow red areola, has formed on the site of No. 10 inoculation.

May 29th.—No. 10 inoculation almost perfectly circular, and stationary. Mr. Richardson made an eleventh inoculation, below and in a line with No. 8, with pus taken from No. 10 sore. The copper lotion to No. 10.

May 30th.—No. 11 inoculation slightly elevated, but apparently not inflamed.

May 31st.—No. 11 inoculation pustulating, but having only a trace of areola.

June 1st.—No. 11 inoculation slightly inflamed.

June 3rd.—No. 11 pustule larger; its areola has increased in width. Patient has some redness of fauces, and there is a small superficial ulcer on the right tonsil. A twenty-grain solution of nitrate of silver was applied to the fauces. But little trace of the copper-coloured rash.

June 5th.—Ulcer of tonsil smaller and cleaner. No. 11 pustule has broken.

June 12th.—She was seized with vomiting and severe pain in the back, the pulse being rapid and the skin exceedingly hot. A few hours after the onset of these symptoms, dusky coloured papules appeared on the face, trunk, and extremities; while at the same time herpes broke out upon the lips and septum of nose. These symptoms were suspected of being variolous. She has good vaccine marks upon the arm.

June 13th.—The papules are pustulating.

June 14th.—The nature of this attack being no longer doubtful, she was transferred to our Fever Hospital, and was placed under the care of Dr. Head.

On June 15th—Many of the variolous pustules were umbilicated and most characteristic; several of the others, however, did not suppurate.

On June 16th—All the syphilitic inoculations were found to be healed; and on June the 24th, she was convalescent.

In a few weeks afterwards there was no trace of the syphilitic eruption, and in two years afterwards she continued free from any ostensible symptom of syphilis.

It had better be mentioned that another patient was seized with variola in the hospital the same week that it showed itself in M. T.

Syphilization — Failure of the Inoculations in causing decided Ulcerations.

F. K., aged seventeen years, admitted to the Adelaide Hospital, Dublin, on the 5th December, 1865.

Two large suppurating sores with sharp jagged edges occupy each side of the vaginal orifice, and the urethral orifice is likewise ulcerated. There is considerable purulent discharge from the vagina, and she suffers much from *ardor urinæ*. Mr. Richardson took some of the matter from the surface of the left sore and inoculated her with it on the outside of the right thigh.

December 6th.—Site of inoculation red and papular.

December 8th.—Inoculation has pustulated, and has a red areola nearly an inch in width.

December 10th.—Inoculation continues pustular. Mr. Richardson made a second inoculation about two inches above No. 1, with matter taken from the pustule of the latter.

December 13th.—No. 1 inoculation pustule has nearly disappeared. No. 2 is inflamed.

A circular sore, the size of a threepenny piece, has formed on the outside of the left labium. It is slightly indurated, and is covered with a tenacious milky-looking layer. Mr. Richardson made, with some of this milky-looking secretion, a third inoculation a little below No. 1.

The vaginal sores are improved.

December 14th.—No. 2 inoculation slightly inflamed. No. 3 more so than it. No. 1 has almost entirely disappeared.

December 15th.—No. 3 inoculation inflamed. No. 2 apparently aborting.

December 18th.—Nos. 1 and 2 inoculations have healed. No. 3 only very slightly inflamed.

December 19th.—No. 3 inoculation elevated. Mr. Richardson made a fourth inoculation with some lymph-looking secretion taken from the ulcer of the left labium.

December 21st.—Inoculation No. 3 stationary.

January 2nd, 1866.—No. 3 inoculation aborting.

The sore of outside of left labium has healed without leaving induration. The sore has likewise disappeared from the orifice of the vagina. The three were treated with the copper lotion. A small sore discovered a little distance within the vagina on this morning. The copper lotion ordered to be used as an injection.

February 12th.—The vaginal sore has healed.

She left hospital on her own accord, probably because she found herself so improved in general health, and because of the cure of the sores.

She has not since reported herself at hospital.—B. W. R.

Syphilization—Great Improvement during the Treatment—Almost complete disappearance of the eruption when the patient left Hospital.

J. L., aged twenty, an anemic, delicate-looking man, with a senile

appearance, was admitted to the Adelaide Hospital, Dublin, on the 3rd of April, 1868.

His body was covered with groups of papules of a slightly copper colour, which were desquamating. Some erythematous patches were upon the face, and over the scalp, and he had pains in the crests of the tibia. Both tonsils were enlarged and red. When he was about a week in hospital an abscess formed in the left tonsillitic region, which was opened by Mr. Richardson. A large quantity of pus was thereby discharged.

One of the post-cervical glands of the left side was enlarged, and there was an enlarged gland in each sub-maxillary region; and now there are several very hard enlarged glands in each groin.

A portion of the glans penis has been destroyed, an indurated cicatrix occupying this part. This destruction of a portion of the glans had its origin in an ulcer which formed a week after coition. He states that it had been burned with a blue acid (nitrate of copper?). The rash appeared five weeks after the appearance of the ulcer. He has gonorrhoeal discharge also.

May 2nd.—Mr. Richardson inoculated him over the third intercostal space of the right side, a little internal to nipple, with matter taken from a suppurating sore of one of the dispensary patients.

May 4th.—Inoculation has pustulated, the pustule being surrounded by a red areola. Pulse 88.

May 5th.—The pustule is very large and full, the surrounding areola being wide.

May 6th.—Pustule larger. Mr. Richardson inoculated some of the matter taken from it, a couple of inches lower down the chest.

May 7th.—Pustule No. 1 surrounded by a larger areola. No. 2 inoculation pustulating. Pulse 76.

May 8th.—Pustule No. 1 scabbing; no apparent alteration in No. 2.

May 9th.—Pustule No. 1 has increased in area. Pustule No. 2 advancing slowly.

May 10th.—Pustule No. 1 larger. Pustule No. 2 has become more active. Pulse 112.

May 11th.—Pustule No. 1 apparently subsiding by the scabbing process. Pustule No. 2 has increased in area. The pains of the tibia have much diminished.

May 12th.—Mr. Richardson removed the scab of pustule No. 1, and exposed an oval ulcer about one-third of an inch in its long diameter.

Pustule No. 2 is larger, and has a broad areola. Mr. Richardson inoculated the patient with matter from this pustule, a couple of inches distance from it.

May 13th.—No. 1 ulcer scabbing; No 2 a little larger; No. 3

inoculation "taking," being elevated and surrounded by an areola. To discontinue the iodide of potassium mixture.

May 15th.—Mr. Richardson made a fourth inoculation a little distance from No. 3, with matter taken from No. 2 ulcer.

May 20th.—Mr. Richardson made a fifth inoculation near No. 4.

May 21st.—Tibial pains are again troublesome.

May 22nd.—No. 4 inoculation the seat of a well-formed pustule. Tibial pains not so severe.

May 25th.—Mr. Richardson made a sixth inoculation over second rib of left side, with matter from Nos. 4 and 5. Pains in tibia have ceased.

June 1st.—Mr. Richardson made a seventh inoculation about three inches above left nipple, with matter from No. 5.

June 3rd.—Mr. Richardson made an eighth inoculation about two and half inches below No. 6 inoculation.

June 9th.—Mr. Richardson made a ninth inoculation below No. 8.

June 10th.—Rash has nearly disappeared from the back, and altogether from the front of the body, and his health appears to be improved in every respect.

June 11th.—No. 9 inoculation having aborted, and Nos. 6, 7, and 8 not producing any decided local results, Mr. Richardson made a tenth inoculation with matter taken from a well-marked secondary ulcer of a patient in an adjoining bed, not being able to procure the matter of a primary sore.

June 12th.—Mr. Richardson made an eleventh inoculation with matter from the secondary ulcer alluded to.

June 16th.—No signs of activity in inoculations Nos. 10 and 11. Mr. Richardson made three inoculations on his chest to-day with matter taken from a sub-pustular ulcer of the same patient, being the 12, 13, and 14 inoculations.

June 20th.—Mr. Richardson made two inoculations, on the patient's left side (15 and 16 inoculation), with matter taken from the same sub-pustular ulcer that afforded the matter for Nos. 12, 13, and 14.

June 29th.—Mr. Richardson made two inoculations (17 and 18) on left side with matter from the sub-pustular ulcer.

All these inoculations with matter taken from the secondary ulcer have aborted.

July 3rd.—Must leave hospital, his employers having given him notice to return to his work. He is wonderfully improved in general health, and has barely a trace either of eruption or enlarged glands.

[In the following August he continued in seemingly good health.—B. W. R.]

Syphilization—Almost complete disappearance of the Rash, and great improvement in the general Health during the process.

R. W., aged eighteen years, was admitted to the Adelaide Hospital, Dublin, on the 25th March, 1868. He originally had a sore surrounding the orifice of the penis, which was followed by a well-defined rash, copper-coloured, mixed with some papules, and which were desquamating on his admission to Hospital. The eruption at this time was situated upon the trunk, arms, shoulders, and lower extremities. There are several hard, enlarged glands in the posterior cervical region and in the groins; and there is some cicatricial induration around the urethral orifice. The fauces are injected.

20 grain frictions of ung. hyd.

May 20th.—Mr. Richardson discontinued the frictions, and inoculated him over third rib of left side, with matter taken from a suppurating sore on the penis of a dispensary patient.

May 22nd.—The inoculation has pustulated; but the latter, having been accidentally broken, an irregular ulcer has been thereby exposed. It has a red areola.

May 25th.—Mr. Richardson made a second inoculation, a couple of inches below No. 1, with matter taken from the latter.

May 27th.—No. 2 inoculation, at the end of the forty-eighth hour, was not so developed as No. 1 had been at the expiration of the twenty-fourth hour.

June 1st.—Mr. Richardson made a third inoculation, a couple of inches below No. 2, with matter taken from No. 1.

June 3rd.—Although Nos. 2 and 3 have been successful, they have not developed so rapidly as No. 1. Mr. Richardson made a fourth inoculation, a couple of inches from the first, with matter taken from one of the pustules of J. L., whose case has been just described.

June 9th.—No. 4 inoculation has pustulated and ulcerated. Mr. Richardson made a fifth inoculation with matter from one of the pustules of J. L.

June 11th.—He is much improved in health and in appearance, and has a good appetite. There is no sign of rash on the front of his body, and it has perceptibly faded on his back. Mr. Richardson made two inoculations (6th and 7th), above right nipple, with matter taken from Nos. 4 and 5 inoculation sores.

June 16th.—Mr. Richardson made three inoculations (8th, 9th, and 10th), above left nipple, with matter taken from the sores of Nos. 6 and 7. None of the inoculations have healed, No 1 being still in existence, although healing. They have all run the same course. A pustule formed at each, and in none was its formation delayed beyond the forty-eighth hour. Each pustule burst and then scabbed. When the latter

was raised from each, a circular-cupped sore, having its surface covered with pus, was exposed.

June 23rd.—Mr. Richardson made three inoculations (11th, 12th, and 13th), over left breast, with matter taken from one of the ulcers of J. L.

June 29th.—Mr. Richardson made two inoculations (14th and 15th). [The report does not say, however, whence the matter was procured, and it has escaped my memory. They were made below the nipple of right breast.—B. W. R.]

July 2nd.—Mr. Richardson made two inoculations (16th and 17th) on right hypochondrium. No return of rash on front of body, and that of back is very indistinct.

July 6th.—Nos. 14, 15, 16, and 17, inoculations not having developed into such characteristic ulcers as their predecessors, Mr. Richardson procured some lymph-looking secretion from a tolerably indurated, but a suppurating, sore, situated on the dorsum of the prepuce of one of the dispensary patients, and he made two inoculation (18th and 19th) with it on the left part of epigastrium.

July 7th.—Nos. 18 and 19 inoculations elevated, having red areola, and are apparently pustulating.

July 13th.—Nos. 18 and 19 inoculations have pustulated and scabbed. They are surrounded by a red areola. On removing the scab from each, an ulcer was exposed having an elevated margin, and about the size of a fourpenny piece. One is covered with pus, the other is not so purulent, but is granulating. Mr. Richardson made two inoculations (20 and 21) over the right hypochondrium, with matter taken from each, but he did not use both with each inoculation.

July 24th.—Mr. Richardson made two inoculations (22 and 23) with matter taken from Nos. 20 and 21, which have pustulated and ulcerated. The latter being circular, excavated, and about the size of a fourpenny piece.

July 30.—Nos. 22 and 23 have pustulated and ulcerated, each ulcer having a red areola. The ulcers are about the size of a fourpenny piece, are not indurated, have a sharp elevated margin, and secrete a thin yellow pus. Mr. Richardson made two inoculations (24 and 25) with matter taken from No. 22 on the right side of the abdomen, about two inches from No. 22.

August 3rd.—Nos. 24 and 25 have pustulated and ulcerated, but they are slower in their development than any of the others were. A small yellow patch has formed on the left tonsil. Ordered an alum gargle.

August 4th.—The patch has disappeared from the left tonsil.

August 6th.—Nos. 24 and 25 ulcers continue small. Mr. Richardson made two inoculations (26 and 27) on the right side of the abdomen, with matter taken from Nos. 24 and 25; and he made a third inoculation (28)

on the left side of the abdomen, with some lympho-looking secretion taken from an indurated sore of two months standing, situated upon the prepuce of a dispensary patient.

There is a small movable indurated gland in the right axilla.

August 12th.—No 28 having succeeded, Mr. Richardson made an inoculation (29) with matter taken from it, on the left side of abdomen.

August 19th.—Mr. Richardson made three inoculations (30, 31, and 32) on left side of chest, with matter taken from No. 28, both of these having been successful.

Sent to convalescent home for a few days.

August 27th.—Nos. 30, 31, and 32 have succeeded. [When he returned to hospital they were nearly healed. He had no relapse of eruption on the front of the body, and there was scarcely a trace of it on the back. He was so much improved in general health that he resumed business. I have not seen him since.—B. W. R.]

ART. VIII.—*Researches on the Sounds of the Heart.* By GEORGE PATON, M.D., Toronto, Canada.

IN every class of animals in which we have examined the action of the heart, we have found that the ventricle contracts towards the orifice of the aorta or vessel that conveys the blood from the ventricle into the system, the mode of contraction being evidently designed to facilitate the passage of the blood from the respective ventricle into the aorta or pulmonary artery.

In contracting, the ventricle does not continue its action till the whole of the blood is expelled. It only contracts to an extent proportionate with the impetus received from the auricles, expelling a certain amount of blood, termed the blood-wave.

June 29th.—Took a large turtle, and removed a portion of the sternum or bone that covers the thorax, leaving the thoracic parietes uninjured; and observed the action and listened to the sounds of the heart.

When the ventricle contracts and propels the blood into the aorta, it raises up slightly the parietes of the thorax, and we can distinctly follow the course of the blood from the ventricle into the arch of the aorta, by the heaving up or elevation of the muscular parietes. There appears to be two impulses or movements, first, that of the contraction of the ventricle; second, that of the distension and reaction of the aorta, which take place simultaneously, and are synchronous with the first sound of the heart.

The heart having been fully denuded, the animal continued strong and vigorous, moving about with great energy, and survived several days. Pulsations, 32 per minute.

We applied the stethoscope, and listened to the sounds of the heart. Both sounds are distinctly heard: the first—a dull and prolonged sound terminating by a sort of knock, or as if a piece of cloth or leather were fully stretched out, or extended to its ultimatum—is heard most distinctly over the semilunar valves, at the origin of the aorta, or a little above it, and synchronous with the contraction of the ventricle, and pulsation of the aorta. The second sound immediately follows the first; is a short, sharp sound, like that produced by the tongue striking the roof of the mouth, and occurs during the contraction of the auricles, as they pour their blood with force into the ventricles during the diastole of the latter.

When the ventricle contracts, the blood passes rapidly into the aorta, which becomes more curved and tense, the contraction proceeding from the apex to the base, and as it reaches the muscular fibres around the origin of the aorta, they contract with energy, passing below the semilunar valves, constricting the part, sending the last portion of the blood-wave into the aorta with force; the aorta starts up, and the rapid reaction of the distended parietes causes the blood to recoil against the semilunar valves and shut them, as it imparts an impulse to the wave. The fibres of the ventricle being contracted behind the aorta, as if supporting the valves, whilst the blood recoils against them—synchronous with which the first sound of the heart attains intensity and terminates.

The sound commences as the blood passes with force through the aortic foramen, and terminates in the aorta, as the blood is thrown back against the valves and shuts them, whilst the aorta pulsates. Sometimes a little roughness or bruit attends this sound, which disappears as the ventricle contracts with greater vigour. The sound is distinctly heard through the medium of the stethoscope, or by the naked ear applied over the heart, and as many as 30 pulsations can be counted at a time without interruption.

Immediately after the contraction of the ventricle and impulse of the aorta terminate, the auricles contract; and as they propel their blood into the ventricle, the second sound of the heart is heard. It is a short, sharp sound, distinctly recognized through the medium of the stethoscope. It appears to be deeper seated, and not so near the ear as the first sound, and is heard at the side of the aorta

in the position of the auricles pouring their blood into the ventricle. The whole of the auricles now contract, and not a part of them, and as they pour their blood with force into the ventricle, the second sound of the heart is produced.

We performed similar experiments on several other turtles, during the highest temperature of the season, and with precisely the same results. In some cases, the action of the heart amounted to 36 pulsations per minute; in others to 40, and even 50, per minute. But in general 36 to 40 pulsations per minute were the highest attained.

The ventricle in contracting propelled its wave of blood with force into the aorta, distending its walls, and the aorta reacting with a power superior to that by which it was distended,^a caused the blood to recoil against the semilunar valves, and shut them, as it imparted its impetus to the wave—the ventricle being contracted behind the valves, when the impulse of the aorta is exerted, synchronous with which the first sound of the heart is distinctly recognized over the semilunar valves, or a little above that. The impulse of the aorta preceding the pulse at the extremity of the arteries by a very short interval.

The fibres of the ventricle relax as the auricles contract, and the parietes of the aorta at the same time become straighter, and attain an interval of rest, till the ventricle contracts and again distends the aorta, there being the closest connexion between the condition of the ventricle and that of the aorta.

Dr. Hope, as the result of his experiments on the sounds of the heart, concluded that the first sound depends on three causes. First, a degree of valvular sound, produced by the blood recoiling against the auriculo-ventricular valves; second, a sudden jerking extension of the muscular walls of the ventricle, as a sound is produced by the extension of the leather of a pair of bellows, which he termed the sound of extension; third, a prolongation or increase of this sound by the sonorous vibrations peculiar to muscular fibre. Dr. Williams maintains that the first sound of the heart is produced chiefly by the muscular contraction itself, and that, as a general law, a sound accompanies every rapid muscular contraction. The Dublin Committee, as the result of their experiments on the sounds of the heart, concluded that the first sound is produced either by the rapid passage of the blood along the internal surface of the ventricles on its way to the mouths of the arteries, or by the

^a Poisseuille.

bruit musculaire itself, or probably by both those causes combined; whilst the London Committee attributed the first sound to muscular contraction, but considered that the impulse of the heart against the thorax may occasionally act as an auxiliary cause, in increasing the intensity of the sound. M. Cruveilhier, as the result of his observations, states that the first sound is heard most distinctly at the origin of the large arteries, and diminishes as we approach the apex of the heart. And Carpenter concludes that the principal cause of the first sound exists at the entrances to the arterial trunks. And it does not seem, he states, that any other reason can be assigned for it than the prolonged rush of blood through their orifices, and the throwing back of the semilunar valves, which, in suddenly flapping down again, produce the second sound.^a

From these statements, it appears that physiologists have, in general, confined the first sound of the heart to the action of the ventricle; but that M. Cruveilhier and Carpenter believe that it is connected with the entrance of the blood into the aorta and pulmonary artery—a view which we consider the nearest approach to the real cause of the first sound of the heart.

We must further observe, it is considered an established fact in physiology, that the first sound of the heart is synchronous with the contraction of the ventricle, and also with the pulsation of the aorta, as it is termed, with the pulse of the arteries near the heart. Carpenter says that “the first sound is evidently synchronous with the impulse of the heart against the parietes of the chest, and also with the pulse as felt near the heart.” Dr. Wood^b states the “first sound is heard during the contraction or systole of the ventricle, and is synchronous with the beating of the ventricle, and with the pulsation in the large arteries near the centre of circulation, but anticipates by a very minute, but still appreciable interval, the pulse of the wrist.” Müller observes,^c “the pulsation of the arteries near the heart is synchronous with the systole of the ventricle;” and Dr. Hope maintains^d that, synchronous with the systole of the ventricle are, the first sound, the impulse of the apex against the ribs, and in vessels near the heart, the pulse; but in vessels at some distance, as the radial, the pulse follows at a barely appreciable interval. From this united testimony of physiologists,

^a Carpenter's Physiology, p. 419.

^b Wood's Medicine, Philadelphia.

^c Müller's Physiology, p. 699.

^d Hope on the Heart, p. 56.

it must be admitted that the first sound of the heart is synchronous with the contraction of the ventricle and pulsation of the aorta.

But the aorta cannot pulsate without the blood recoiling against the semilunar valves, and shutting them, as it is impelled onwards. If the blood had been sent along the aorta in a constant stream, there would have been no shutting of the valves in its onward course. But the blood is propelled by the ventricle *par saccades*, in successive waves, and as each wave is sent with force into the aorta, it distends its walls, and the aorta instantly reacts, shutting the valves by the rebounding of the wave as it is impelled onwards. And the ventricle is fully contracted at the origin of the aorta, as if supporting the valves, whilst this impulsive movement takes place. The distending force of the ventricle ceases as the last portion of the wave enters the aorta; but it is now within the valves, and the parietes return on their contents with an energy superior to that by which they were distended, throwing the blood against the valves and shutting them, as it exerts an impulse on the wave; the impulse of the aorta being succeeded by the pulse at the wrist by only an appreciable interval of about one-thirtieth of a second.* Hence it is during the onward course of the blood that it recoils against the aortic valves, just as the fluid in the forcing pump reacts against the valves, during the lateral pressure that propels it forward.

We have seen, in the course of our experiments, that the first sound of the heart is heard most distinctly over the semilunar valves, at the origin of the aorta, and occurs as the aorta pulsates, and the valves shut. During the ventricular systole the blood is sent rapidly into the aorta, rendering it more curved and tense; and as the contraction reaches the muscular fibre at the origin of the aorta, they contract with energy, passing behind the semilunar valves, constricting the part, whilst the aorta becomes fully distended and reacts, throwing the blood against the valves and shutting them by the impetus given to the wave. Immediately after this, the ventricle commences to dilate as the auricles contract, and the parietes of the aorta grow straighter, and recover their former situation, till the ventricle again contracts, and the movements are repeated.

These facts seem to establish the proposition that the blood recoils against the aortic valves when the ventricle is contracting—not when it is dilating; and that the sound which is then produced

* Müller's Physiology, p. 176.

is the first sound of the heart. It commences as the blood is propelled with force through the aortic foramen, and attains its intensity in the aorta as it reacts on the advancing wave. It is a dull and prolonged sound, synchronous with the contraction of the ventricle and impulse of the aorta, and precedes the pulse at the extremity of the arteries by only an appreciable instant.

And we can perceive the necessity of the semilunar valves being shut, and sustained by the ventricle contracted behind them, as the full impetus of the distended aorta is imparted to the blood-wave—the impulse recoiling against the valves and passing along the tube with such rapidity that the amount of blood which enters the aorta, almost simultaneously displaces a similar amount at the extremities of the arteries—communicated to the veins.

In contraction, the force which the ventricle exerts is spent on the impetus or momentum given to the wave, and the distension of the aortic parietes; but as an artery always reacts with increased vigour, the latter force is more than regained, and exerted during the arterial systole.

No portion of the first sound could be connected, during these experiments, with the auriculo-ventricular valves, because a membranous expansion, very small, of the internal parietes or living membrane of the ventricle is extended over the orifices, and covers the part completely during the contraction and expansion of the walls of the ventricle; and it is so small, and so situated, that no sound can be produced at the part during the ventricular contraction.

The facial artery, says Müller, which is known to depend on the contraction of the ventricle, is nearly synchronous with the heart's impulse, being only one-thirtieth of a second later than it, while the second sound is not heard until one-fifth of a second after the impulse; consequently, the second sound can in no respect be connected with the influence of the aorta. "The second sound," according to Dr. Hope, "results from the sudden expansion of the semilunar valves, occasioned by the reflux on them of the columns of blood in the aorta and pulmonary artery during the ventricular diastole." And Dr. Williams says:—"The second sound being caused solely by the sudden reaction of the arterial columns of blood on the semilunar valves, its loudness will depend on the mobile and perfect state of these valves, and the extent and abruptness by which they are stretched by the recoil of the blood at the moment of the ventricular diastole. It will therefore be most perfect when the heart acts regularly and slowly, giving time for the

full gush of blood to carry the valves loose into their slight recesses in the walls of the artery, and for the as perfect reaction of the contents of the distended artery on their concave surfaces." ^a But if the second sound be produced by the columns of blood reacting against the semilunar valves when the artery is distended, that must occur during the ventricular systole, for there are no columns of blood in the aorta and pulmonary artery during the ventricular diastole. The blood has already been propelled forward by the pulse in the aorta and pulmonary artery during the contraction of the ventricle; it is then only the blood that remains in the contracted arteries—that is, in the arteries after the systole has taken place—that can fall back and recoil against the valves during the diastole. Dr. John Reid, whose statements are more explicit on this subject, says:—"Synchronous with the first sound of the heart we have the impulse of the heart against the chest, and the propulsion of the blood along the large arteries." And during the second sound, which is synchronous with the diastole of the ventricle, we have the regurgitation of part of the blood in the large arteries upon the semilunar valves, throwing them inwards to the axes of the vessels. And as the second sound appears to be produced by the shock of the blood upon the semilunar valves, its intensity must in a great measure depend upon the diastole of the ventricle drawing part of the blood back upon them; but perhaps more particularly upon the elasticity of the large arteries returning suddenly on their contents during the diastole of the ventricle, when the distending force of the ventricle has been withdrawn." ^b This observation shows that Dr. Reid, who performed numerous experiments on the action of the heart, considered that the reason usually assigned by physiologists for the cause of the second sound was insufficient to account for it, and therefore, he says, perhaps more particularly upon the elasticity of the large arteries returning suddenly on their contents. But we must observe, the ventricle, during its diastole, cannot, by a sort of suction power, draw the blood back upon the semilunar valves with a force sufficient to produce an acute and sharp sound like the second sound of the heart. Besides, the dilation of the ventricle always commences at the auriculo-ventricular foramen, and when the action of the heart is vigorous, the contraction of the auricles is synchronous with the diastole of the ventricle, and nothing like a suction power then

^a Williams on the Heart, p. 213.

^b Cyclopaedia of Anatomy and Physiology, London, Art. Heart, by Dr. John Reid.

exists in the ventricle to produce a reflux of the blood in the aorta against the semilunar valves. And as regards the elasticity of the arteries returning suddenly on their contents, that occurs during the systole of the ventricle, when the blood is impelled forwards. During the ventricular diastole the arteries straighten themselves, and the aorta feels soft and compressible, and is in a state of rest, till its parietes are distended by the next ventricular contraction. And, in the course of our experiments, no sound was produced in the aorta during the ventricular diastole.

Physiologists seem to think that the contraction of the ventricle propels the blood along the arterial tube to its extremity, independently of the action of the semilunar valves; and that the blood in the distended arteries recoils against the valves during the diastole of the ventricle, and produces the second sound of the heart. But this is not the manner in which the heart acts,* and it is evident that if there were columns of blood in the aorta that recoiled against the semilunar valves during the ventricular diastole, it would act as an obstacle to the blood entering the aorta, and require an additional force to be exerted by the ventricle during its contraction to overcome it; and it would allow no period of rest to the parietes of the aorta, as they would be kept distended, both during the ventricular systole and diastole, which would speedily destroy their action.

The second sound of the heart has no connexion with the aorta, but depends on the contraction of the auricles, and the force with

* The ventricle does not act like a syringe, forcing the blood along the arterial tube to its extremity, and immediately relaxing, whilst the blood in the distended arteries recoils against the semilunar valves, as that would be reversing the movement which had just been completed. But in transmitting the blood from the heart through the arterial system, the ventricle acts in concert with the aorta and its valves; and for the sake of illustration, we may be permitted to allude to the action of the single forcing pump, which it somewhat resembles. The piston, which is contained in the working barrel at the base, has a range of two feet or upwards, and, on being raised, immediately descends to that extent, the valve opening, and the fluid passing readily into the upper compartment of the tube. But the instant the upward stroke is given, the valve shuts by the fluid recoiling against it, and a distinct sound is produced. And as the piston is elevated to the limit of its range (viz., two feet), it exerts a force upon the whole of the fluid contained in the tube, expelling at the upper extremity an amount of fluid equal to what had entered at the base; and when the ventricle contracts, it sends with an impulse the wave of blood into the aorta, distending its walls, and the aorta, reacting on the blood that has entered it, forces it against the valves, shutting them, and produces the first sound of the heart as the impetus is exerted on the wave, the impulse of the aorta being synchronous with the contraction of the ventricle, and preceding, at a very short interval, the pulse at the extremity of the arteries.

which they pour their blood into the ventricles; and occurs immediately after the first sound terminates.

We have seen that closely following the first sound of the heart, a second sound is heard, of a short, sharp, and acute character, like the second sound of the heart in man, and occurs during the contraction of the auricles and diastole of the ventricle; as was clearly ascertained by counting 1, 2, 3, 4, 5, 6, &c., whilst we listened to the second sound through the medium of the stethoscope; and several gentlemen carefully observed the contraction of the auricles, and each contraction of the auricles exactly coincided with the second sound as heard by us. Pressure on the aorta above the semilunar valves, at the moment of the occurrence of the second sound, did not in the least diminish its loudness and intensity, but it was affected by whatever impaired the action of the auricles; when they became weak, the second sound was scarcely audible, but when they contracted with energy, the second sound was clearly and distinctly recognized. It appeared to be produced at the auriculo-ventricular foramen, by the force with which the blood is propelled from the auricles into the ventricles. Sometimes it partook of a slight bruit, as if the blood passed over something rough on entering the ventricle. But when the auricles contracted with vigour, the sound was sharp and acute, like the sound produced by the tongue striking the roof of the mouth. It appeared deeper seated, and at a greater distance than the first sound, because the auricles are here deeper seated than the arch of the aorta, and it did not follow the course and direction of the first sound. The first sound ascended along the aorta, but the second descended by the side of the base of the aorta, in the situation of the blood passing from the auricles into the ventricle. We tested this question with the greatest possible care, and distinctly ascertained that the second sound of the heart is produced by contraction of the auricles, as they pour their blood with force into the ventricles. After its occurrence, a momentary silence ensued, and then the first sound recommenced.

Dr. Hope concluded, as the result of his experiments on the sounds of the heart, that no sound is produced by contraction of the auricles. But this depended on the manner in which the action of the heart was maintained. We are informed that during these experiments, in which artificial respiration was employed, the auricles contracted only partially, or chiefly in their appendices. But to produce a distinct sound, the auricles require not merely to

contract, but to contract with energy, in expelling the blood from the cavity. We have found, in the course of our experiments, that when the action of the heart was slow and weak, a sound was scarcely heard, during the contraction of the auricles. At other times it was not constant and continuous, but when the action was vigorous a sharp sound was produced, distinctly recognized through the medium of the stethoscope, closely following the first sound. The principle which chiefly contributes to the production of sound in the contraction of the auricles is what obtains as a general law in the passage of fluids through narrow or contracted orifices. If the force by which the blood is propelled be weak, little or no sound is heard; but if the blood be propelled with energy, a clear and distinct sound is produced.

And Dr. Williams informs us, "that in some recent experiments with Mr. Clendinning, he found the auricles of an ass produce a very distinct sound when they contracted vigorously, and independently of the ventricles: this was afterwards heard by all who were present. The same phenomenon has also been observed in some experiments recently performed in America."^a Drs. Linnock and Moore heard a sound produced by contraction of the auricles.^b And we have had ample evidence of this fact in the course of our experiments, during these two last summers, on the action and sounds of the heart in the American turtle.

Much has been said by physiologists respecting the second sound not being produced by contraction of the auricles, from the time at which the auricles contract. Harvey, Lancisi, Senac, and Haller, considered that the auricles contract immediately before the contraction of the ventricles, and not immediately after the termination of the preceding contraction. Dr. Hope and Dr. Williams also maintained this doctrine as the result of their experiments; and Professor Turner, arguing on these data, showed that as the second sound closely follows the first, it could not depend on contraction of the auricles, as their contraction immediately precedes that of the ventricles. Now, it is a fact that, when the action of the heart is slow and weak, as in cold-blooded animals, when it beats at the rate of 20 and 22 pulsations per minute, the ventricle, after contraction, immediately dilates, and blood enters it from the distended auricles, filling it out to a certain extent; and then the auricles contract and produce contraction of the ventricle, the one movement immediately preceding the other. The same thing occurs in

^a Williams on the Chest.

^b Wood's Medicine, Philadelphia.

warm-blooded animals after the thorax has been opened, when the action of the heart becomes slow and irregular, or its movements are interfered with, as by preventing the action of any of its valves. In these cases we have observed that, after contraction, the ventricle immediately dilates to a certain extent, and blood passes into it from the distended auricles, and then the auricles contract, and produce contraction of the ventricle. But we are by no means, from this data, to conclude that this is the manner in which the action of the heart is maintained when it is quiet and regular, beating as in warm-blooded animals, at 60, 70, and 80 pulsations per minute; and in the turtle at 32, 36, 40, and 50 pulsations per minute. We have already shown^a that as the action of the heart increases, the auricles contract sooner in point of time and of rhythm; then their contraction becomes synchronous with the diastole of the ventricle; so that they commence to contract immediately after the termination of the preceding ventricular contraction. And let a physiologist examine the action of the heart when it has been quickly denuded in a warm-blooded animal, and he will see that the auricles contract immediately after the termination of the preceding ventricular contraction is finished, and that the action is maintained with surprising power, the one movement following the other in quick and regular succession. And this is exactly what we have seen in the course of our experiments on turtles during the highest temperature of the season.

In observing the action in a denuded heart, we distinctly perceive that the contraction of the auricles commence the cycle or beat, and the contraction of the ventricle and impulse of the aorta terminate the beat; the one beat succeeding the other in such quick and rapid succession that, when the action is vigorous, the contraction of the auricles is synchronous with the diastole of the ventricle. But in listening to the sounds of the heart through the medium of the stethoscope, or by the naked ear applied to the chest, we associate the dull and prolonged sound with the short and acute sound, considering the former the first sound of the heart, and the latter the second sound, and that they belong to the same cycle or beat of the heart. But the reverse is the case. The short and acute sound is in reality the first sound of the heart, being produced by contraction of the auricles, which commence the cycle or beat; and the dull and prolonged sound is in reality the second sound of the heart, being produced by contraction of the ventricle and impulse

^a British Medical Journal, February, 1868.

of the aorta, which terminate the beat; so that the short and acute sound does not belong to the same beat of the heart as the dull and prolonged sound which precedes it, but is the commencement of a new beat, and must be associated with the ventricular contraction and impulse of the aorta that immediately succeed it; which enables us to see that the short and acute sound may appear to succeed the dull and prolonged sound, but is in reality the first sound of the heart.

We wish it to be distinctly understood that the statements we make respecting the sounds and rhythm of the heart, refer exclusively to the action of the heart when it is quick and regular, and the contraction of the auricle immediately succeeds the contraction of the ventricle.

The first period of silence occurs between what is termed the first and the second sounds of the heart, and depends on the time at which the auricles commence to contract after the contraction of the ventricle and impulse of the aorta terminate. According to Muller it is one-fifth of a second; but Laennec thinks that the second sound begins immediately after the first ends. From careful and repeated observations, we are of opinion that an appreciable interval occurs between the first and second sound—that is, between the termination of the contraction of the ventricle and impulse of the aorta, and the commencement of the contraction of the auricles. But this interval is reduced to a minimum when the action becomes very rapid, the heart beating 100 and 120 times per minute.

The second period of silence occurs between what is termed the second and the first sound of the heart, that is, between the termination of the contraction of the auricles and contractions of the ventricles. It is longer than the first interval, because it extends through the period of the ventricular action, till the blood is sent with force into the aortic-foramen, and the semilunar valves shut.

From what has been stated, we arrive at the following conclusions, respecting the sounds of the heart:—

The first sound is produced by contraction of the ventricle and impulse of the aorta. It commences as the blood is propelled with force through the aortic foramen and attains its intensity in the aorta, as the blood is thrown back against the aortic valves and shuts them on the impulse being imparted to the wave.

This sound is heard most distinctly in the denuded heart over the semilunar valves—at the origin of the aorta or a little above it.

The second sound depends on the contraction of the auricles, and is produced as they propel their blood with force through the auriculo-ventricular foramen into the ventricle during its dilatation.

It appears to follow the first sound as an immediate sequence, as it takes place so quickly after its completion, but it is the commencement of a new beat, and synchronous with the dilatation of the ventricle, and, of course, precedes the ventricular systole.

And as the same principle obtains in warm-blooded animals, the first sound of the heart is produced during the ventricular systole, as the blood is propelled with force into the aorta and pulmonary artery, and attains its intensity as the blood has entered their orifices and is thrown back against the semilunar valves and shuts them, by the re-action of the distended parietes imparting an impulse to the wave, the impulse of the aorta preceding the pulse at the wrist, by an appreciable instant.

The second sound of the heart is produced by the force with which the blood is propelled by contraction of the auricle, through each auriculo-ventricular foramen, into the ventricles during their diastole.

From what has been stated, it will be seen that a bruit sometimes attends one or both sounds, when the action of the heart is weak, and disappears when the action is strong—so that one cause of inorganic murmurs is the want of energy in the contraction of the auricles or ventricles, and whatever restores this removes the murmur.

From the manner in which the first sound is produced, we can perceive how a murmur connected with the contraction of the left or right ventricle is propagated along the aorta or pulmonary artery; a circumstance on which Dr. Hope laid great stress in determining to which side of the heart the murmur belonged

ART. IX.—*Cases illustrating the Use of the Laminaria Digitata Tents.*^a By JOHN A. BYRNE, M.B., Univ., Dublin; Professor of Midwifery to the Catholic University of Ireland; &c., &c

I.—CASE OF INTRA UTERINE POLYPUS—SEVERE HEMORRHAGE—REMOVAL—RECOVERY.

II.—CASE OF RETENTION OF PIECE OF OVUM AFTER ABORTION—SEVERE HEMORRHAGE—REMOVAL—RECOVERY.

ALTHOUGH it is an easy matter to diagnose extra-uterine polypi by the touch and sight, yet when the polypus is internal great

^a Read at Meeting of Dublin Obstetrical Society, June 11, 1870.

difficulties exist in many cases in arriving at a correct conclusion as to the cause and origin of the fearful hemorrhage, which is occasionally the most prominent symptom of its presence. The reason of this is that frequently the entrance into the uterus is so constricted and closed that we cannot pass a sound or uterine probe through the narrow passage, and thus we have no means of forming a correct opinion, unless there be some very apparent enlargement of the uterus, which at once leads to the diagnosis of tumour of some kind. The late eminent Professor Sir J. Simpson, feeling this difficulty, devised the plan of artificial dilatation of the os and cervix uteri by means of sponge tents. He gave the results of his experiments to the profession in a paper read before the Medico-Chirurgical Society of Edinburgh in November, 1849, and thus he may fairly claim to be the originator of the plan of artificial dilatation. Had this great man left no other memoir behind him than this, it would be sufficient to entitle him to the thanks of the profession; but I need scarcely say that his contributions to uterine surgery and therapeutics have been most extensive and varied.

There are, however, certain disadvantages connected with tents made of sponge, such as the difficulty of introduction, their tendency to slip out—as on one occasion I had myself an opportunity of observing, their becoming fetid from the uterine secretions; so that now they are not much used, although, however, we cannot afford to discard them altogether.

Many instruments were subsequently devised for this purpose—such as Dr. Priestly's dilator, on much the same principle as the urethral dilator, Dr. Marion Sim's dilator, and others. These are all, no doubt, useful as temporary means of dilatation, but they fail to afford us the same advantages for operative purposes as Simpson's sponge tent, or that which was introduced to the notice of the profession by Dr. Sloan, of Ayr, in a short memoir published in the *Glasgow Medical Journal*, in the year 1862. This tent, made of the laminaria digitata, or sea-tangle,^a possesses all the requirements of a substance necessary to dilate; thus it is light, clean, easily procured, can be made of any length and size, possesses remarkable properties of dilatation, does not become fetid from absorption of discharges, and is very easy of introduction, and, moreover, is capable of being so managed, that one or more can be

^a For the first application in Dublin of the laminaria digitata tents in the dilation of the uterus, we are indebted to my friend, Dr. G. Kidd, who published an able memoir on the subject in *Dublin Med. Journal*, 1869 (*vide*).

easily introduced into the cervix, so as to occupy it completely, according to the narrowness of the passage which we wish to dilate.

Having had several opportunities lately of testing its peculiar advantages, I have selected from my note-book the following two cases in which its use proved eminently useful in producing satisfactory dilatation of the os and cervix, and thus enabled me to observe the source of long-continued and severe hemorrhage:—

Mrs. M., aged 21, consulted me on 12th October, 1869, for uterine hemorrhage, which had continued some time. Her statement was to the following effect:—She had been married about two years, had one child in April, 1869; when she was three months nursing this child she became pregnant, aborted at three months, had a good deal of hemorrhage; after this she again became pregnant, again aborted at three months, or thereabouts, and since that time has been in very delicate health, having been scarcely ever free from hemorrhage. When she came to consult me she was very anemic and feeble from the frequent attacks. She was a healthy-looking woman in every respect, and with this exception had been always so. Before marriage she had suffered from dysmenorrhea.

On my examining her with the speculum there was no appearance of anything which could account for this—the os uteri was healthy; on examining, however, with the finger, the cervix uteri felt rigid, the os uteri was closed, and I detected an enlargement and hardness of the uterus at the junction of the body with the cervix; the os itself was so contracted that it scarcely admitted the point of the sound, and I could not introduce it for even the smallest distance without causing considerable pain, so that I was obliged to desist. She was very hysterical, and I could not arrive at any conclusion derived from this source of examination. I told her my opinion was that there was a tumour seated within the uterus, and that it was most probably the cause of the hemorrhage, although it would be impossible then to form an opinion as to its curability. I ordered her cold lotions and ferruginous preparations; and, as she then absolutely declined any more interference, I said I could not be of much service to her.

On the 14th inst., just two days from her first visit to me, I was hastily summoned to see her in the evening. She had been suddenly attacked that morning with flooding and had lost an alarming quantity of blood. She had tried the usual remedies—cold, vinegar, &c., but they all failed. When I saw her she was really almost

moribund, her pulse was scarcely perceptible, her lips were pallid, and on looking at the bed I perceived that several large clots were lying in napkins. She was so ill that I was obliged to use the tampon—give brandy freely, gallic acid—and I thus succeeded in checking it for some days. When the tampon would be removed, it would cease for a few days, again return, and several times she was nearly moribund from the great losses which she sustained. After two or three days' cessation of the hemorrhage, she would again acquire strength, her lips would regain their colour, and she, thinking that it would no more return, would get up, move about, and then, after a short interval of freedom from bleeding, she would rapidly lose a large quantity, expelling it partly in coagula, partly fluid; treatment would arrest it for a time, but it would return. Thus from the 14th to the 30th October matters passed. I wished to explore the uterus; explained my views to her friends and husband; but I could not induce them to consent to any operation for the purpose of effecting a permanent cure. In this state of things I told them to obtain other advice, as I would no longer be responsible, and I did not see her for several days, when on the 24th of November I received a note from her husband, stating that she would consent to anything which I proposed. I asked my friend, Dr. G. Kidd, who had seen her for me during an unavoidable absence on two days, and who entertained the same view of the case as I did, to assist me. On the 25th of November he proceeded to explore the uterus. The first thing necessary was to place her under the influence of chloroform, which was carried out effectually by Dr. Kidd for me, as it was utterly impossible to do anything to relieve her otherwise, she was so nervous and unmanageable. This having been effected, I introduced a duck-bill speculum into the vagina, and seizing the anterior lip of the os uteri with a strong vulcellum, so as to fix it, the sound was passed up through the narrow and constricted cervix. In doing so there was some difficulty, which was caused by the tumour; but this difficulty having been overcome, a piece of the laminaria, about four inches in length, was passed through the os, and into the cervix; another was passed alongside this, and so on, until five of them were passed. Dr. Kidd suggested this plan, instead of using those which are sold for the purpose and prepared, on account of the latter being too short; and as our object was not only to dilate the os and cervix uteri, but also the body of the uterus, I fully concurred with him. Having introduced them nearly the entire length, I passed a small plug of

cotton wadding into the vagina, and the first part of the operation was finished.

In a few hours she began to complain of pain over the uterine region, but this was relieved by a suppository of morphia introduced into the rectum.

She slept well on that night, and Dr. Kidd and I met again the following morning.

She was placed in the usual position, and the plug and tents removed.

The os and cervix uteri were so much dilated by the process adopted, that I could easily introduce the index and middle finger of my left hand. On doing so, I felt a soft tumour of about the size of a grape, attached to the anterior and lateral wall of the uterus, near the cervix, and corresponding exactly to the tumour felt on examination previously; it was not pedunculated, but sessile, and very soft and pliable. Dr. Kidd also examined it, and satisfied himself of its nature; intending to pass a loop of wire around it, and extract it with the ecraseur, I grasped it with a vulcellum, but it was so soft and pliable that it came away, and I was obliged to tear it out in fragments; any portion which I could not grasp I scraped with my nails, and having taken away it all, the interior of the uterus, where it was attached, was touched with strong nitric acid.^a

I need not occupy the Society with details as to subsequent treatment. She recovered without a single bad symptom. For two or three days there was a dark-coloured discharge, which appeared due to the contact of the acid.

She was well in a few days. She rapidly regained strength. She had no return of the hemorrhage, and she menstruated regularly until February, when she again became pregnant, and she never has had the slightest annoyance since.

DR. HAYDEN'S REPORT.

"Shreddy portion consists entirely of fibro-plastic structure, *i.e.*, fibre-cells drawn out at extremities into fine filaments with large oval nuclei in central portion. Under strong acetic acid the filamentous portion disappeared in some measure; a few red blood corpuscles were likewise visible. The soft pulpy portion consisted of mucous exudation entangled in the foregoing structure."

On looking at those small broken pieces, which once formed a single mass, it could scarcely be believed that so small a substance

^a Dr. Ringland was the first to recommend the application of nitric acid in these cases.

could give rise to such alarming symptoms as those just detailed; but those who are conversant with such matters are aware that hemorrhage of the most alarming nature is a frequent attendant upon polypi, whether seated within the uterus or external to the os. I have myself seen very severe hemorrhage to arise from the small vascular polypus, scarcely the size of a small pea, which is occasionally seen to be seated just at the orifice of the os uteri, and we know very well the fearful flooding which proceeds from the ordinary pedunculated extra-uterine polypus. I have not the slightest doubt in my mind that this lady would have died in some attack of flooding, or else that she would have gradually become exhausted from the frequent attacks, were it not that we were fortunately able, by exploration of the os and uterus, to ascertain the origin and source of it, and thus remove the cause; and this could not possibly have been effected without the aid of some substance such as the laminaria digitata, whose properties of expansion are so wonderful, and, at the same time, so innocuous.

The second case in which I had an opportunity of testing its efficacy was the following:—

On November 13, 1869, at 2 a.m., I was hastily summoned to see a lady residing at Waterloo-road. When I reached the place, I found the lady in a most piteous condition, the entire bed was saturated with blood, napkins, and everything which could be obtained were full of it; large coagula lay scattered about, and she herself was pulseless—cold—in a state almost of syncope, and, in fact, almost moribund.

Having administered stimulants, and stopped the hemorrhage. I inquired her history. It was this:—She had been some years married; was 32 years of age; had three children, the eldest of whom, a boy, about six, was with her; she had been under treatment in Dublin for some time previously, she told me for uterine complaint, and, according to her statement, had on one occasion a small polypus removed, which had given rise to considerable hemorrhage. Since then she had aborted twice. She recovered very well after her last miscarriage, which took place about three months before; and she was going about in her usual manner. At the next menstrual period she thought that it was more abundant than usual. Soon afterwards she began to notice that she had a discharge every fortnight, then every week, and then every second or third day; rest and cold would stop this, but surely it would come on again. She

began to become alarmed, and had come up from the country that day with the intention of consulting me on the following day. She was a very healthy looking woman; very large, and well formed; of florid complexion, and very strong-minded. She had been so frequently under the care of some of our best metropolitan gynecologists, that she was quite conversant with the names of all the instruments used by us.

The sudden and nearly fatal hemorrhage precipitated her intention of sending for me, and I found her in the condition which I have attempted to depict.

On examination, when she had sufficiently recovered, I found the vagina filled with coagula; but I could detect no cause for such severe hemorrhage. It was metrorrhagia evidently depending upon some internal cause. I applied a large pledget of cotton, steeped in a saturated solution of perchloride of iron, to the os—having syringed her with cold water—then plugged the vagina, administered stimulants freely, and gallic acid and ergot. Next day she was improved; there was no return, and so matters went on for a few days. I examined very carefully; there was no ulceration of any kind—simple or malignant; there was not the slightest abrasion even. I examined her with the sound; it afforded no positive information; the uterus was normal in size and position.

She soon recovered from the effects of the hemorrhage, and regained strength; but in a few days she was again attacked, and lost a considerable quantity of blood. I determined now to explore the os uteri; and, with this intention, I introduced a piece of sea-tangle of large size, and left it in for 24 hours. It produced no inconvenience. On the next day, when I removed it, and passed my finger into the cervix, I found a small piece of something tipping against my finger, which, on removing my finger, fell out into the vagina. I need scarcely say that there was no return of the hemorrhage. In a few days she was quite restored to health; and was delighted to get rid of her annoyance, which she examined herself.

On examination of the substance expelled, it proved to be a piece of the fetal membranes, consisting of thickened chorion and amnion, which latter was quite evident from its white glistening appearance, and which had remained from the previous conception and abortion. The lady has returned to the country, and I believe, but of this I am not certain, that pregnancy has taken place.

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9. *On the Origin of Species by means of Organic Affinity.* By H. FREKE, A.B., M.D., T.C.D. 1861. London: Longman & Co.
10. *An Appeal to Physiologists and the Press.* By H. FREKE, A.B., M.D. Dublin: Fannin & Co. 1862.

“LET the earth bring forth” was, we read, the expression of the Almighty's will when initiating the creation of organic life on the

globe; and that word, never spoken in vain, is followed in the inspired record by the narrative that the earth did bring forth abundantly of living creatures. To this expressed will of God, as the Final Cause, all investigations in Biology, when read rightly, seem to point us. But it were not accordant with the spirit of Philosophy to rest satisfied with this knowledge of ultimate fact alone, as long as nature spreads out before us her varied phenomena, inviting us not only to the study of the *by whom*, but as emphatically to that of the *how*.

It were equally contrary to the spirit of true philosophy to be content with a simply phenomenal acquaintance with nature, and either to refer natural events to the direct interposition of the Divine Power, irrespective of any plan, or, on the other hand, to negate *Ætiology* altogether, and to rest satisfied with that which we know positively and by our senses. In the former case we would impute to the Almighty an unmethodical system of working, and in the latter we would disregard some of the plainest and most emphatic of the inferential teachings of nature. "It is no mere theory, but a fact, that creation has had a history—that this work, as it has been pursued in time, has been pursued by method."—(Reign of Law.) Our consciousness is so conditioned that we cannot think of any phenomena as absolutely beginning or ending. Before it, we look for some power producing; after, we look for some result produced. Whenever we thus find phenomena so related to each other that the first is always followed by the second, and the second always follows and depends on the first, we regard the first as a cause and the second as an effect: and as we cannot think of this cause as starting spontaneously at any one point in time, we suppose the existence of chains of causes springing from the Great First Cause. Studies of these chains of causes are interesting, as they exhibit to us the method of the Divine Artificer in the creation and upholding of the universe. Now, the world displays to us an extensive series of such natural sequences, and by observing and reasoning on these we gain an acquaintance with secondary or intermediate causes.

Some of the most interesting of the questions attracting notice at the present day are those concerning the bearings and relations of secondary causes in the production and development of life and organic nature, and much obscurity and senseless opposition to scientific investigation and speculation has arisen from a confusion of primary and secondary causes. For, while we believe alike in

the inorganic and organic worlds that "nature is but the name for an effect whose cause is God." Yet we also believe that

"In the origin of things,
When all creation started into birth,
The infant elements received a law,
From which they swerve not since. That under force
Of that controlling ordinance they move."

And the manifestations of this force reveal to us the chains of intermediate causation.

The literature of the subject of the causation of life is extremely bulky, as the books named above in this branch of science are but a fraction of those which in late years have been written; and works in this department of speculative biology are often less attractive than one might expect from the interesting nature of the subject, they are so liable to be spun out to prolixity; and any book in which a very little observation or experiment is diluted with a great deal of theory becomes wearisome. This fault is more to be regretted as some of the authors who fall into it have really got hold of good ideas. A speculative author, who wishes his views to be generally taken notice of, must put them forward tersely, with no useless verbiage.

We will commence our investigation of this subject by collecting the different answers given by philosophers to the question, What is the cause of life? and in connexion with this we will examine the views of organization held by authors of different schools.

I. Life, say Aristotle, Stahl, and Cabanis, is the correlative of mind; both are the results of one principle. II. Life, say the followers of Barthez, Hoffman, Blumenbach, Müller, Kämt, Stirling, &c., is the result of a special principle *sui generis*, separate from mind. III. Life, say the followers of Oken, Haeckel, Carpenter, and Huxley, is the sum of the effects of the molecular forces in a complex nitrogenized substance called urschleim (Oken), or cytoplasm (Remak), protoplasm (Schultze), bioplasm (Beale). Writers of the second class personify this force as "vitality," "organizing agency," "vis medicatrix naturæ," "Bildungstrieb," "organizing atoms," &c.

On a correct answer to this question grave scientific results depend. If life be the result of the molecular forces, in any form of matter, then its generation is a pure matter of chemistry; and we may expect, as organic chemistry progresses, to be able synthetically to produce the "elementary life-stuff" directly from its inorganic, or,

at least, unorganized elements. Then, also, organisms may arise spontaneously, whenever, in the great laboratory of nature, forces combine to produce protoplasm. Then, also, the life of every organism, being identical in essence and the essential life-material being the same in all—the difference between one species and another is but the result of modifying forces acting from without. The history of organic life on the globe, on this hypothesis, will be divisible into the following parts:—1st. The combination of inorganic materials to form protoplasm; 2nd. This continuing undecomposed, and undergoing a molecular modification in form, becoming what is histologically called a cell, the first step in organization; 3rd. This mass inducing a catalytic change in other primitive inorganic materials, whereby it converts these into a substance like itself. These steps once gained, organization is fully in action, and the process of development of various organic forms is but a matter of time.

Those who believe in the vital-principle doctrine hold that life has no essential relation to the other known forces of nature, is incapable of generation by any process of chemistry, not being a chemical or physical force—that it is, in reality, an agency antagonistic to these forms of energy. Life, says Bichat, is the sum of the functions resisting death. Life, says Lamarck, is that order of things in the parts of each body possessing it that renders the performance of organic action possible, and effectually resists death. In these definitions, by death is meant chemical decomposition, *i.e.*, physical force in its ordinary manifestation in flesh. Hence, in this view, we cannot understand the nature of life by any analogy drawn from the ordinary forms of energy manifested in inorganic nature, and the same forms of life may or may not be the common possession of all living beings. Hence this doctrine does not pledge its adherents to any theory of origin of species.

These are the conclusions which logically follow from either of these theories. It will now be interesting to inquire what are the grounds upon which it would be possible inductively to arrive at either theory.

In obtaining data upon which to reason, the special points on which we seek to obtain information are the following:—

1st. Has the force of life any similarity to the other forces of physical energy, are these identical in essence, and is any other of the forces of nature convertible into it, or *vice versa*?

2nd. Have we any facts to show us the possibility of the

spontaneous generation of organized living beings, or is the reverse established?

3rd. Is the elementary material of every organized being identical in chemical composition, properties, and functions?

4th. Has this elementary life-stuff (if such exists) any inherent essential organization?

5th. Have we any evidence in favour of the origin from one common protoplasm of various organic forms?

If we can obtain reliable and satisfactory elucidations of these points we will have no difficulty in deciding to which hypothesis they direct us; for if life is similar to, derivable from, and convertible into other physical forces, if organisms are capable of arising spontaneously, if organization is always associated with a definite chemical compound with an inherent method of growth and capable of developing in different directions, then we are shut up to the hypothesis that life is the sum of the effects of the molecular forces in protoplasm. If we obtain direct negatives of these facts then we may adopt the other doctrine.

Preliminary to these inquiries it would be of the deepest interest to examine what grounds we have for rejecting or accepting the Stahlian theory of the identity of life and mind; and, in order to do so, it is necessary to know what we mean by the term mind used in this connexion by the followers of Cabanis and Stahl. If by mind we mean the capacity of reasoning or of drawing conclusions from premises, then undoubtedly it is hard, if not impossible, to separate the phenomena of life from those of mind; for there is no doubting the fact that many of the more intelligent of the lower animals, contrary to the dogmas of the Cartesian school, do associate ideas, and infer one judgment from another, any one will find numerous illustrations of this in any work on the habits of animals; and the gradations from the most unintelligent to the most intelligent of the lower animals are too imperceptible to allow us to draw a line. But all, except the most materialistic of philosophers, cannot fail to admit that man exhibits capacities of self-improvement, not only in degree but in kind, far above any other animate being; and a living French naturalist, while referring to the want of any positive structural difference between man and the anthropoid primates, mentions that there are two qualities possessed by him which at once separate him from the lower creation, and both of these—the *Moralité* and *Religiosité* of De Quatrefagés—spring from a common root, from the feeling of accountability inherent in man, shown in his almost

universal adoption of some object or idea of worship. If we inquire in what part of man's nature these qualities inhere, analogy fails to lead us to an answer; for we have in organic nature no analogue, and in the absence of aid from without we can only guess at the seat of such feelings, for "who knoweth the spirit of the sons of man that is ascending and the spirit of the beast that goeth downward to the earth?" Without the aid of Revelation we cannot state their difference; but we find in the page of Scripture that into the animal man God breathed the special רוּחַ יְהוָה, or נִשְׁמַת breath of life, and man became perfected as the complete נִשְׁמַת; a living soul;^a not merely an animal, he was that by construction before, but he received directly from his Maker the soul, that which converted the material with its physical forces into the perfect man, the dual in unity, thus fitted for his place at the head of the material creation by forming a link between it and the higher immaterial created intelligences.

It may be objected that on this hypothesis (that the development of mind as a specially human attribute is the soul), we cannot draw a sharp line of demarcation between the parts of human intelligence due to vitality (vital principle or molecular force) and those due to the soul. The answer is to be found in the perfect union of the purely psychical and purely physical to form the concrete Ego; the result of which is that our consciousness is a condition not of the soul alone, but of the Ego concrete; and attempts to isolate the consciousness of the soul from the state depending on the condition of the material frame are futile and unphilosophic, for the elements which make up the Ego are in detail unknowable. "To lay down the dualism of the subject, and object as complete and absolute (that is, an out-and-out duality which is not also a unity) which psychology not unfrequently does, is to extinguish every glimmering of the scientific reason, for this implies that the dualism is laid down in cognition as complete and absolute, which it can only be when intelligence acts in opposition to its own necessary and insuperable laws."—(*Ferrier's Institutes*, p. 235.) A disregard to this inseparable unity of active cognition in the Ego is a common cause of the use of materialistic formulæ and modes of thought in expressing doctrines of cerebral physiology, as exemplified in the following:—"We are conscious of an image so long, and only so long, as the

^a In the Targum of Jonathan Ben Uzziel the "living soul" is called the "inspiration of a speaking spirit" (*Memallela*) showing that this was understood, not as referring to man's animal life, but to his special human endowments.

cell or its atoms continue in motion, and our consciousness cease when the cell again becomes quiescent, but disturb the cell's equilibrium again and the image is reproduced, however slight the motion. The cell becomes active whenever motion of another cell is communicated to it; the current or vibratile motion inducing material change in the cell it reaches, very similar to that which took place in the one from which it proceeded, though in a less degree, while the appreciable phenomenon is the faculty of the mind which we call memory. . . . Every idea we have in our memories has its location in a certain number of impressed cells close together."—(*Dickson's Matter and Force, &c.*, p. 9.) The theory embodied in this extract implies that the Ego is a condition of matter and its attribute force purely. In Mr. Stirling's critique on Professor Huxley's lecture on Protoplasm we find that he directs special attention to the materialistic tendency of the view advocated by the professor, although Mr. Huxley defends himself from the charge by saying—"I, individually, am no materialist, but, on the contrary, believe materialism to involve grave philosophical errors" And this he shows by the following argument:—If we suppose knowledge to be absolute, that we know more of cause and effect than a definite order of succession of facts, and know the necessity of the succession, then we must adopt materialism and necessarianism, but we cannot prove that anything may not be the result of a material and necessary cause. Now, the object of science is to find out the causes of phenomena, and there is no difference between the doctrine that life is a product of a certain disposition of material molecules and the old notion of a governing Archæus ruling the blind matter in each body, except that here, as elsewhere, law has devoured spontaneity; but we know nothing of matter except as the name of an unknown cause of our state of consciousness, nor of spirit, but that it is a name for an unknown and hypothetical cause of state of consciousness also; both are the imaginary substrata of groups of natural phenomena. This explanation is deemed unsatisfactory by Mr. Stirling; but if we (believing in the record of the gift of the soul to man by his Maker) take into account the duality of the Ego (thus constituted), of which in unity consciousness is a state, we can easily deliver ourselves from the "materialistic slough" in a much more satisfactory manner than does Mr. Huxley.

Having now briefly considered the bearing of theories of life on

mind, we will see what light facts and observation throw on the subjects of inquiry propounded above.

The first question with regard to the relation of physical and vital force was ably discussed by Dr. Carpenter, of London, in a paper on "The Mutual Relations of Vital and Physical Force"—(*Philosophical Transactions*, 1850, p. 727.) The universal adoption of the doctrine of Mayer and Joule of the correlation of physical forces gave an impulse to the search after connexions between life and other forms of energy. Mr. Grove, in his classic work on the subject, in 1846, states that he believes his mode of reasoning might be applied to the organic as well as the inorganic world; and that muscular force and organic developments of heat at some time will be found to have definite correlations. Mayer, in *Comptes Rendus* for 1845 (p. 385), applied this principle to the phenomena of life; and, later, Dr. Freke put forward the view (1848) that the phenomena of active vitality are produced by the antagonism of two forces, chemical action and organizing agency (*Organization*, p. 22), a theory of which we have some foreshadowing in the remark made by the father of philosophy on digestion—"Neither is digestion, by means of which nutrition is accomplished in animals, done without the psyche (vital principle), nor without heat, for all things are accomplished by fire."—*Aristotle on Breathing*, chap. vi.)

This idea Dr. Freke expands in his theory of the existence of organizing atoms, a progressively advancing chain of materials which he believes to exist, whose function is to act on a corresponding chain of organized structures fitted to receive organization; but to this we will return.

The conversion of physical forces into life is the main point brought out in Dr. Edward Haughton's little book on vital force, and is used by him as a basis upon which to found some very satisfactory principles for medical practice; and as the physiological fact, that we are continually taking in force from without, is universally admitted, there is no necessity to dwell longer on this point. It will suffice to refer for one most valuable series of observations on a branch of the subject to the lecture "On the Relation of Food to Work," by the Rev. Samuel Haughton, M.D., delivered before the British Medical Association in 1868. The converse of this, or the mutation of life into physical force, is equally acknowledged. "Life is a system of vital forces, and the conception of such force involves a fundamental idea. Mechanical, chemical, and vital forces form an ascending progression. Chemical affinity includes within

it mechanical force, and . . . vital forces include in their nature both chemical affinity and mechanical forces, for vital power produces both chemical change (as in digestion) and motion which imply considerable mechanical force.”—(Whewell, *Philosophy of the Inductive Sciences*, Vol. I., p. 34. 1840). In this connexion it is scarcely necessary to allude to the evolution and production of motion, heat, light, and electricity as instances of life changed into correlative forces. [For observations on the subject of the organic evolution of light I would refer to a paper by Dr. A. W. Foot, in the *Proceedings of the Dublin Nat. Hist. Soc.*, 1870.] Nor need I refer to the very interesting researches of Helmholtz on the rate of transit of nerve-force (90 feet in the second), the same as the rate of transmission of sound-waves along a substance of the same density as nerve tissue or softened india rubber. In the face of such observations as these a physiologist will scarcely endorse the words of his Grace the Duke of Argyll, “we know nothing of any of these (vital) forces in the same sense in which we do know something of the force of gravity, or of magnetism, or of electricity, or of chemical affinity. These are all more or less known, not indeed in respect to their ultimate nature, but in respect to certain measures and methods of their operation. No such knowledge exists in respect to any of the forces which have been concerned in the production of life.”—(*Reign of Law*).

These are but a few of the lines of observation which bear on the first question, and they lead us clearly to answer that there is undoubtedly not only analogy between life and the physical forces, but as life can produce or be converted into other forms of energy, and *vice versâ*, we must believe in an essential identity. So plain is this deduction from the structure of electric organs, that we find Goodsir, a firm believer in the existence of a special vital principle, appending to his description of the electric organs of several fish, the following remark:—“The presumed correlation of the nervous and electrical forces in no way trenches on the psychical department of physiology, and has no tendency to exclude the psychical or proper vital element from the science of organization.” But he does not attempt to show how this can be the case.

The second question for consideration is, having seen that life has some analogy to the physical forces, what evidence have we in reference to the question of its spontaneous production? Can we produce living germs, by any combination of physical circumstances?

And here we find observers in natural history grouped into two classes, the one following the chemist Pasteur, and holding the impossibility of heterogeny; the other group following Pouchet, and believing spontaneous generation to be proved. The latter class includes many eminent names in science. Professor Owen in his last work (*Anatomy of Vertebrates*, Vol. III., p. 817), states boldly his acquiescence in the view. Hæckel, Spencer, Joly, and Musset, Pouchet, Schwann, Schultze, Hughes Bennett, and others have likewise ranged themselves as heterogenists. These latter rely on positive experiments (too numerous to detail here, but a full account of which will be found in the little work by M. Pennetier, *Sur L'Origine de la Vie*, and in Dr. Bastian's very able paper in "*Nature*, Vol. II, p. 170, Facts and Reasonings concerning the Heterogeneous Evolution of Living Things,) made, under all conceivable circumstances, in Rouen, Turin, London, on the Alps, on the Pyrenees, with all manner of infusions to which air is admitted through boiling water, through concentrated sulphuric acid, through caustic potass, through red-hot tubes, where all manner of confessedly exaggerated precautions have been taken; and yet, despite all, organisms have been produced. Infusions of the same substance, in the same place, in different depths of vessels, produced different living beings, and, as Treviranus showed, by altering the substance infused we get an alteration of organisms. In short, varying conditions of air, temperature, substance and electricity, in the same place, vary the produced organisms. Whenever a putrescible substance is exposed to moisture, a moderate temperature, and air, there living beings appear; therefore, either these organisms are propagated by invisible germs, of whose existence we have not a particle of evidence, and they are able to resist temperatures fatal to every known organism, or else heterogeny is proved; but bacteria and vibriones, and others of the Protista, are killed by a heat of 70° cent., as Victor Meunier has shown, the same heat which kills vorticellæ and paramecia; and the eggs of paramecia are killed by a heat of 75° c. (Pouchet); so from analogy we would expect such a heat as 75° c. to kill any living germs; but 100° c. has failed to kill these hypothetical germs of vibriones and bacteria, or at least to prevent the development of life.

The counter evidence mainly rests on M. Pasteur's experiments, made in similar conditions, in which he found no traces of life; but the magnifying power used by him was one of 350 diameters;

while Dr. Child (P.R.S., 1855,) found that in eight out of thirteen experiments a magnifying power of 1,500 to 1,700 diameters was needed fully to investigate the produced life-forms in closed vessels. In cases like this one well established fact of positive evidence is worth a hundred of negative; but for the many experiments of such reliable and earnest naturalists on the side of heterogeny as Jeffrys Wyman, Pennetier, Mantegazza, Pouchet, Joly, and Musset, Meunier, Child, Schwann, Schultze, Needham, Bastian, &c., we have on the side of panspermism the experiments of Pasteur producing negative results, but which results were manifestly sought for by inadequate means.

We now come to the consideration of the most interesting questions which we have propounded for study, those concerning the existence of a common "*lebenstoffe*" in all organized beings, and its inherent properties.

That the essential principles in all animal bodies are very similar has long been known; and although the substance protein, described by the Dutch chemist, Mulder, is not believed by chemists at the present day to be the basis of these compounds, yet the commonly received idea among the majority of chemists is that these bodies are nearly, if not absolutely, identical in ultimate chemical composition, being mayhap allotropic forms of an extremely complex substance, probably an organic acid, one of whose forms is living protoplasm. The most essential of these allied elements, albumen, fibrin and casein are undoubtedly convertible. The first has been proved to be transmutable into the second by the well-known experiments of Smee; and the converse of this metamorphosis, I am informed by Dr. Reynolds, is accomplished at the present day, on a large scale, by the prolonged maceration of fibrin in a very dilute solution of hydrochloric acid. Casein is similarly transmutable; and these facts show to us that probably these three substances, so closely related in composition, may be but varieties of one central material.

To this chemical identity of all forms of protoplasm Mr. Stirling makes four objections—1st. That even if the composition be identical allotropy may introduce an element of variations; 2nd. That Mr. Huxley's analysis is that of dead protoplasm, and as the composition of this *may* differ from that of living, the analysis is indecisive; 3rd. Secondary products often exist in protoplasm cells; 4th. All forms of protoplasm do not exhibit the same reactions on applying tests. The first and fourth objections are not very weighty.

Allotropic forms of any body vary in properties, but are convertible; and if the protoplasm, or "cellstoff," of organized beings present different properties while they have a similar ultimate composition (and the property-differences among forms of protoplasm are extremely slight), that is no argument against there being a unit protoplasm of which these forms are variety; so that Kühne's observations on the intolerance of cold by *Amæba*, while the trout ova furrow famously in iced water and are killed in a warm room, is no argument against the essential resemblance or identity of these as protoplasm, or against their being derived from the same source, especially when we consider that the same observer has shown that the same chemical conditions are found in the two cases, and that Dr. Ransom has proved that fish ova motions are affected by external conditions precisely as *amæba*.—(*Journal of Anat. and Phys.*, Vol. I., p. 241). That cells contain secondary products is not any objection to the theory of unity of essential basis, if these products can be shown to be chemically derivable from an albuminoid body. Mr. Stirling enumerates protagon, myosin, cholesterin, glycogen, &c., which he says introduce chemical difference; but in the case of several of these materials, their affinities to each other and to the albuminoid group of bodies is a matter which has been proved. Thus Dyblowsky has shown that the material described by Stricker as cholin is identical with neurin, a product of the decomposition of Liebreich's principle protagon (*Journal für Pract. Chem.*, p. 153. 1868.) De Jaffé has proved the convertibility of bile pigment into urine pigment (*Centralblatt*, No. 2. 1869). Preyer, the identity of the hæmoglobin of all blood-possessing animals (*Pflügers Archiv. für Phys.*, 1868. p. 395). Salkowsky, the identity of hæmatoidin and bilirubin. Professor Heynsius, of Leyden, has demonstrated the identity of globulin and albuminate of potassa. Hoppe-Seyler, Burdach, Kemmerich, Voit, and Ssubotin show that fat (one of the secondary products whose presence is supposed by Mr. Stirling to destroy the unity of protoplasm) is the product of the retrograde metamorphosis of albuminoid bodies. These are a few of the most recent chemical observations on this subject, and many more might be cited, pointing one way, towards the chemical identity of source in these derivatives from protoplasm.

The albuminoid bodies are a group by themselves, of difficult and obscure affinities, whose main character is instability. This is the part of the subject which, from the attendant chemical difficulties, is as yet most obscure; but everything that we do know is

in favour of the belief that all forms of protoplasm are albuminoid, that all albuminoid bodies are identical in material, and only differ as possibly allotropic forms of one substance. All the differences described by Wurtz, Béchamp, and others, can thus easily be accounted for; and, as an eminent modern chemical writer has said, the trivial discrepancies in the composition of these albuminoids are most likely the results of such errors as, even with many precautions, are liable to creep into the most careful analysis.

Not only the chemical but the physical uniformity of protoplasm has to be ascertained, and to belief in this, Mr. Stirling objects, because protoplasm may be homogeneous or granular, and it may be of various shapes, therefore it is not always the same; but the identity contended for by the physicists, is an identity of origin and of essential nature; burnt clay may be a brick, a tile, a cup, a flower-pot, but it is still burnt clay. These are different in texture and form, so is protoplasm; but their nature is the same, and they may be taken from the same lump, so the great Potter, by the wheels and furnaces of his secondary causes, which he has set agoing, as His instrumentality may take the elements of fish or fowl from the same material, and mould them in diverse forms. The substance which we call formative or germinal material or protoplasm is the same in properties, whether taken from parts which are to be developed as tendons, yellow elastic tissue, cartilage, epithelium, the cell contents of nettle-hairs, or the bathybius of the Atlantic bottom; so that though "there is nerve-protoplasm, brain-protoplasm, bone-protoplasm, &c," yet they are as protoplasmata indistinguishable, and are only named from an actual tracing in an anticipatory sense, so that it is a gratuitous assumption to predicate that there is no one of these but produces its own kind, and is uninterchangeable with the rest; for we find one germinal mass becoming bone, and if, in its earlier stages, we compare it with any other mass of formative material, we detect no difference, physically or chemically. In cases of this kind the *onus probandi* rests upon those who assert the non-identity of all protoplasm; and when we inquire for their proofs, they show us the results, and then assume that their assigned *cause* is proved. This is the weak point in Dr. Freke's organizing atoms, and in the theory of the vitalists that the tissues are presided over by an inherent Archæus. In all forms of protoplasm and its congeneric albuminoids, we have a far greater unity of character than in the different forms of carbon, and that in the complex molecular changes, in such

a compound and unstable substance, chemical transformations should occur is what might be anticipated. Each of these closely related forms of albuminoid has special properties. Albumen has certain qualities and properties by virtue of its molecular forces. Fibrin has others. Casein others; and that the albuminoid of germinal matter should be the seat of other molecular forces, is what we must expect. The death of this substance would then be its transformation into some of its allotropic forms, which are much more prone to still further decomposition. That we are not far from organization as a manifestation of the natural forces of protoplasm may be easily inferred from the properties of some of the other kindred albuminoids; thus, when we consider such curious molecular changes as those which take place in connexion with the chemical union of such albuminous materials as the fibrinogenic and fibrinous-plastic elements, we are not so far from phenomena as complex as many of the life phenomena of the Monera. The conclusion to which we are thus brought is that protoplasm is a uniform or granular albuminoid body, identical in appearance in all animals, and in all parts of all animals, before it becomes organized. And thus, though all the houses may be diverse in arrangement and in the texture of their bricks, yet they are all made of the one clay. To the objection that our analyses are of dead protoplasm, it may easily be answered that when we find the same changes of condition affecting the varying kinds of living matter in the same way, it is a fair ground for a presumption of unity, when we cannot, from the nature of the case, submit living protoplasm to analysis.

In connexion with our studies of the chemical and physical identity of protoplasm, we may, with profit, examine what we can learn regarding the method whereby protoplasm becomes modified in organization, that is, the natural form which protoplasm masses tend to assume; a subject which is very well discussed in Dr. Tyson's admirable book on *The Cell-Doctrine*, by far the clearest, most concise, and complete little work on the subject yet produced in English.

The discovery by Schwann and Schleiden of the nucleated cell, as the basis of organization, was the first step towards the establishment of a true scientific understanding of Histogenesis, and taking this cell (though not in the sense used by either of these authors) as our primitive structure, we find all organisms to be made up of these elements. But what is a cell? Schwann describes it as

consisting of an external *cell-wall* containing protoplasmatic material called *cell contents*, in which is a central denser mass or *nucleus* containing a *nucleolus*; this nucleus was supposed by him, as afterwards by Goodsir, to be the centre of nutrition and of cell-growth; each cell may multiply, according to Henle, by—1, budding; 2, endogenous cell development; or 3, by segmentation, as in the *Enschnürung* and *Furchung* of the yolk.

According to Goodsir "the nucleus is the permanent source of successive broods of new cells." According to Professor Hughes Bennett, cells are mere clusters of molecules with no special parts, a result of the organization of molecules, and the special parts, cell-wall, and nucleus are the results of subsequent differentiation. Virchow, the physiologist, to whom, after Schwann and Schleiden, the cell theory owes most, re-echoes the essentials of the cell as described by Schwann. The contents execute the function, the nucleus presides over life; according to him, each cell has its own separate activity, each its own surrounding territory, over which it reigns. One group of cells, those of connective tissue, is "the starting point of physiological and pathological processes," and these cells by transformation become modified into elongated canaliculi by the disappearance of the contents and the elongation of the wall; connected to these canals, according to Chronszczewsky and Afonnasiew, and in relation to these corpuscles, the radicles of the lymphatics arise; by these the beaker cells of the intestines communicate with the lacteals; and the canalicular pore systems of the arachnoid, pleura, synovial-membrane, and peritoneum described by Böhm, Recklinghausen, and Dyblowsky, are most probably formed by them.

But that the nucleus is no necessary part of the cell is easily seen, as Brücke shows (*Die Elementar Organismen*, &c., p. 18–22), in the cells of some cryptogams, as sphagnum, hydrodictyon, &c. Stricker refers also to its non-essential character in the fecundated egg of the frog, &c., and in the prothallus of some ferns. In the "organisme ohne organ" of Hæckel, Protamœba, Protogenes, &c., the Monas amyli, and Protomonas of Cienkowsky (*Schultze's Archiv.*, 1865.) In Schultze's non-nucleated Amœba porrecta, and others of the Monera. As long ago as 1840, Reichert referred to its absence in the developing yolk-cells; in all these no nucleus exist. All these, being examples of living cells without nuclei, demonstrate clearly that the nucleus is no essential part of the cell.

Neither is the second element or the cell-wall an essential, for Pringsheim (*Untersuchung über d. Bau d. Pflanzenzellen*, 1854),

showed that there was no such membrane as the primordial article, essential in the vegetable-cell, specially differentiated from the subjacent protoplasm. Franz Leydig, in 1856, stated the same fact for the animal cell, that no nitrogenous cell-wall was essential to it; and Max Schultze, in 1861, defined a cell as protoplasm surrounding a nucleus. Those of the Monera, before referred to, are, for the most part, as destitute of a cell-wall as of a nucleus. So these considerations reduce the essentials of a cell to the one element alone, a mass of protoplasm. Still we find, despite this absolute demonstration of the existence and growth, and life of cells without nuclei, prejudice is so strong that many writers cannot reject this cherished organizing Archæus; thus we find such an ingenious writer as Mr. Stirling saying, "And this is the view to which I, who have little business to speak, wish success. I cannot help believing that this nucleus itself, as the analogue of the subject, will yet be proved the most important and indispensable of all the normal cell-elements."

To this theory of the essentials of a cell, it is no objection that the nucleus has been seen to move independently, even when removed from the cell, by Böttcher, because if the nucleus be a mass of protoplasm, motion is just what we might expect in it, for as Hessling and others have shown all forms of protoplasm exhibit motion (*Grundzüge der Gewebelehre des Menschen*, 1866, p. 25).

According to Beale, the cell consists of two parts germinal or formative material, and formed matter; the first is protoplasm in its simplest form, with an inherent power of producing matter like itself; the nucleus is the new centre of germinal matter, the nucleolus, a still younger centre, and the *nucleoleolus* a still younger; formed material Beale regards as dead structured, a phraseology in which he is very properly corrected by Professor Humphry and Dr. Tyson. For although, on Beale's theory, it is not growing *per se*, yet it is function-performing, these germinal masses may be of various sizes, but seldom do we find masses over $\frac{1}{120}$ th of an inch, which do not segment into smaller particles, varying from the $\frac{1}{10000}$ th to the $\frac{1}{200}$ th of an inch, and even to larger sizes.

Dr. Tyson's views mainly agree with those of Beale, and he holds the vitalist doctrine in opposition to the "Urschleim" hypothesis of Huxley, Oken, &c. He believes in the possession by germinal matter of an inherent power of motion, as illustrated and described in white corpuscles by Cohnheim, Addison, &c.; but regards it as

presided over by a special personified agency (vitality or vital force).

The processes by which histogenesis takes place, starting from the first form of life, or that of the non-nucleated mass of "life-stuff" seems to be:—1st. The concentration of a portion of this material in the form of a nucleus or denser central mass, whose vital activity, however, has never been proved to be greater than that of the surrounding germinal matter or protoplasm, except in respect of its greater density, the outer portion of the protoplasmic mass being condensed becomes the cell-wall; growth takes place by the conversion of cell-food into germinal matter by a chemical process, whose details we know not yet. Cells grow and multiply by the methods described so long ago as 1843, by Henle; and the further changes by which the primitive cells become differentiated into the various tissues, can be studied either in the developing egg or in the gradations of the animal kingdom. All authors do not look upon protoplasm masses as thus developing; but the vast majority are in favour of such a process. Professor Hughes Bennett regards molecules as the ultimate elements of organic tissues, and believes growth to depend on the successive formation of histogenetic and histolytic molecules, a view in which he is forestalled by Dr. Freke, who, in 1848, says that the organization or formation of an organic entity is necessarily accompanied by the simultaneous disorganization of another.

The theory that to the cell-material rather than to the cell-structure belong the properties of organization and life, is one to which we are compelled to adhere by a study of the Monera; and reminds us of the quaint utterance of Oken, that the higher animals were clusters of monads, and their so-called death was their ultimate resolution into these components. There is no necessity for a belief in special dynamical particles (organizing atoms—Freke), for if we regard the indistinguishable protoplasm of organization as an unity, the natural stimulants of heat, light, electricity, &c., would be amply sufficient to stimulate the organizable into the organized.

If it be true that the protoplasm of different animals in the egg is indistinguishable from the "urschleim" of the Monera, as shown by Kühne, Huxley, and Hæckel, and admitted by Beale, and that the materials of the different tissues in their embryonic state, are indistinguishable in composition and properties, it forms a strong *a-priori* argument in favour of a community of origin of species, and hence the "physicists" are of necessity believers in development. A

study of morphology presents us with so many facts inexplicable on the ordinary teleological theory, that at the present day there are few comparative anatomists who have not given in their allegiance to evolutionism. A few of these most striking grounds of the Darwinic hypothesis are well put forth in Mr. Flower's introductory lecture, in which he shows that the teachings of morphology can be interpreted but one way, and that a correct classification is but a means of exhibiting genetic affinities. All animals of each class are built on a common plan, parts useful in one and useless in another, are still present in the latter, although wasted and abortive. Such structures, if animals were constructed on the principles of pure teleology, would not be present; and to say that animals are built on a common type, explains nothing, but that "the Creator had imposed certain apparently quite arbitrary restrictions to His power; but beyond this almost paradoxical assertion it gives no clue to elucidate anything like a theory of Creation."—(*Flower*, p. 13). The theory that all animals have sprung from one original germ is not confined to the followers of Darwin, for long before the publication of the "*Origin of Species*," in 1861, Lamarck, in 1801, put forth a development theory, which he perfected, in 1809, in his "*Philosophie Zoologique*." Le Maillet, the author of the singular work, "*Le Telliamed*," published in the beginning of the last century, also proposed in that book a development hypothesis, which was advocated in 1849 by the author of the anonymous "*Vestiges of Creation*." Earlier still, in 1794, Erasmus Darwin, in "*Zoonomia*," had suggested evolution, as also did Herbert in 1822, Grant in 1826, Matthew in 1831, Freke in 1851 (for a complete sketch of the history of this doctrine see Haeckel, *Natürliche Schöpfungsgeschichte*, p. 89, *et seq.*) Into the merits of this theory our present space forbids us to enter, but it will suffice to refer to it as far as is necessary to inquire into the claims of Dr. Freke as an original observer in this branch of speculative physiology, meanwhile referring those who wish to study the most systematic exposition of Darwinism to the work of Professor Haeckel, of Jena, above quoted. Dr. Freke claims to have established, by induction, the theory at which Mr. Darwin has attained by analogical reasoning. But in Dr. Freke's work there is no reference to natural selection, to the struggle for life, or the causes of variation which form the main subject of Darwin's theory; for, as His Grace the Duke of Argyll has very properly stated, Darwin's theory deals more with the method of variation than with the fact

of variation, and the only thing in this connexion to which Dr. Freke can lay claim, is the proposition of an evolution theory. It will be seen that Professor Haeckel puts his theory into its true historical place (*Schöpfungsgeschichte*, p. 96). Dr. Freke's theory of the method of evolution is this: that there exists "a chain of progressively advancing organizing agents or atoms, whose function is to confer organization . . . we believe in an equal necessity before that function can be fulfilled, for the existence of a corresponding chain of progressively advancing organized structures, whose function is to receive organization, and which were designed and adapted for calling into operation the function of the organizing atoms" (*Organization*, p. 40, 41; *Appeal*, p. 17). "Organic creation universally (viz., all animals and plants,) admits of being reduced to two ultimate physiological divisions, namely, simple germs and the organic tissues developed by these germs; the former I have termed organizing atoms, and the latter organized residual products." These views, published in 1848, were undoubtedly original; but, if we understand them aright, while they are the foreshadowing, yet they can scarcely claim to be, as Dr. Freke hints, the parallels of the views of Dr. Beale, published in 1861. According to Freke, the organizing atom, a substance different from the material upon which it acts, elevates the latter one step in the scale of organization, then becomes acted upon itself, "remodels its own type," and forms the organized residual product. On the other hand, according to Beale, matter may be in two states, germinal or formed; the germinal is active, capable of multiplying itself, and it is the material which becomes self-transformed into the formed matter. But the question naturally arises what evidence have we of the existence of such special organizing atoms as Dr. Freke describes. A particle of protoplasm is converted in the process of development into muscle. Why? Dr. Freke answers, because it is acted upon by the organizing atom, musculat; but what evidence is there of this? Dr. Freke has not given to us a single original experiment or microscopic observation or analysis in his entire work on the "*Origin of Species*," nor are his quoted facts, experiments, or observations more numerous, as may be seen from the fact that, with the exception of a single mention of the names of Schwann and Schleiden, scarcely a physiological author is referred to in the work. This is the great weakness of the book, and we have little doubt but that this is the reason why less notice has been taken of Dr. Freke's books than they deserve. He deserves credit for many

original ideas; thus, although his views are not as referred to above, precisely similar to Beale's, yet he undoubtedly was among the first who drew a line of demarcation between *forming* and *formed* matters, the forming matter, or organizing atom, having in its character something of the nature of Goodsir's "*Centres of Nutrition*" (1845). At p. 83 of the "*Origin of Species*," and in the *Medical Press* for 1851, he most unequivocally propounds the doctrine of Pangenesis; further developed later by Darwin. The main weakness of the theory arises from: 1st, the hypothetical distinct nature of the organizing atoms, for of their existence we have given to us by Dr. Freke no direct evidence, as to support the doctrine of their existence, our author falls back on the theory of "specific stimulation," and here his induction becomes analogy (*Origin of Species*, p. 16; and, 2nd, the want of adduced facts or observations in support of the theory. This latter weakness is very clearly evidenced in his appeal, in the comparison instituted between his views (1852), and those of Mr. Savory (1861). The principle involved in both statements is the same, and therefore Dr. Freke is entitled to the priority; but Mr. Savory appeals to facts, and his statement is a record of observations. Dr. Freke makes a corresponding statement with no appeal to observation.

Dr. Freke's books show him to be a man of great ability and of originality. The first of his works, or that on organization, is one which deserves to be closely studied, and if to the important theoretical idea put forward in it, he had added any practical confirmatory observations, we have no doubt but that his book would have long since take a foremost place in physiological science.

The object of putting forth writings on such subjects as those above referred to, should be the elucidation of phenomena and the discovery of the truth; and in an essentially practical subject like that of any branch of morphology it is fact not hypothesis that is required; hence the force of such writings as those of Darwin, Wallace, Fritz Müller, Dohrn, Haeckel, Flower, Huxley, and Beale, which contribute new facts to the general store. The question of development must be decided by work in the dissecting-room, the field, the zoological garden, and the laboratory; and no authors can expect to receive credit for their opinions but such as by the eye, the scalpel, the microscope, or the test-tube, can demonstrate what they assert, or, at least, the existence of a tangible ground for their assertion.

Even to the present day there lingers a shred of old prejudice

against researches in the principles of biology, but surely there is little necessity to affix any apology for views on these subjects in relation to their bearing on revelation. The days are gone by when it was counted heresy to believe anything not expressed in the pages of Holy Writ, or to interpret scientific facts therein recorded otherwise than literally. The battle fought and won by astronomers against the churchmen was renewed between the geologists and the so-called defenders of Revelation; and now when geology has triumphed, the same narrow-minded opposition has declared war in the field of biology; but when we carry with us as the fundamental canon of interpretation in such matters, that natural phenomena are only made use of in Scripture as illustrations, and consequently are expressed in popular and not in scientific language. We free ourselves from all difficulties in this respect, and the better that we can understand the works of the Divine Architect in the department of organization, the more heartily will we join in the apostrophe of the Psalmist:—

לֹא-נִכְחַד עֲצָמַי מִמָּוֶךְ אֲשֶׁר-עָשִׂיתִי בִפְתָר רִמְמָתִי בְּתַחֲתוֹת אֲרֶץ :
 גִּלְמִי ! רָאִי עֵינֶיךָ וְעַל-סִפְּרֶךָ כֹּלָם יִפְתָּבוּ יָמִים יִצְרִי וְלֹא אֶחָד
 בָּהֶם : ^ב

ALEXANDER MACALISTER.

The Physiology of Man, Designed to Represent the Existing State of Physiological Science as applied to the Functions of the Human Body. By AUSTIN FLINT, Jun., M.D., Professor of Physiology and Microscopy in the Bellevue Hospital Medical College, New York. Vol. III. New York: D. Appleton and Co. 1870. Pp. 516.

As, it may be remembered, the two former volumes of this work on physiology, which appeared in 1866 and 1867, were reviewed at considerable length in the pages of this Journal, it is unnecessary now to do much more than refer to what we then said, and to state that the present volume is not behind the others in matter and style. It is devoted to the consideration of the important subjects of Secretion, Excretion, Ductless Glands, Nutrition, Animal Heat, Movements, Voice, and Speech.

^a Undeveloped matter *i.e.* protoplasm.

^b Ps. 139, v. 15, 16.

Although there are many points upon which we would have liked to see more full and accurate accounts given, inasmuch as the treatise before us purports to be the exponent of the existing state of physiological science, and although the papers on the different subjects are unequal in information, yet on the whole we consider it a valuable work, giving a very fair *resumé* of what is already known upon most subjects; while, by the experimental research of the author, some points of great interest, upon which there has been diversity of opinion among physiologists, have received additional elucidation, and new facts, which may possibly prove of considerable value, have been ascertained.

The chapter on the physiology of the kidney is in accordance with the recent works upon most points, although omissions are noticeable—as, for example, in the paragraph upon the pigments of the urine (which have been so fully described by Dr. Schunk), and upon the existence or non-existence of a fibrous stroma.

The glycogenic function of the liver is also very fully described, and Dr. Flint's views, which he founded on experiments performed as late as January, 1869, will tend, it is to be hoped, to settle, to a great degree, and reconcile the heretofore conflicting theories upon this subject.

The chapter, however, upon which it is evident the author has devoted most time and attention, is that in which the functions of the liver with regard to the elimination of cholesterine are discussed. The origin and destination of this substance is most fully considered, at a length, however, which renders the remaining chapters of the volume somewhat short and unsatisfactory. The author endeavours to convince his readers of the purely excrementitious character of cholesterine, the product of nerve waste, and to show that its retention in the blood always gives rise to those severe symptoms which characterize grave icterus, and to which (in accordance with this theory) he gives the name of *cholesteremia*. It will be a most important addition to pathological knowledge if Dr. Flint's views receive future confirmation; we are not in a position to give a decided opinion. We believe that the cases of grave icterus, in which an excess of cholesterine was found in the blood, and which were detailed in Dr. Flint's original paper, in the *American Journal of the Medical Sciences*, October, 1862, are too few to warrant the acceptance of such a theory, and we find that Dr. Lionel Beale—whose opinion on this point deserves every consideration—is much averse to elevating this crystallizable fat into such importance, either physiologically or pathologically.

The Pathology and Treatment of Stricture of the Urethra and Urinary Fistulæ. By Sir HENRY THOMPSON, F.R.C.S.; Surgeon Extraordinary to H. M. the King of the Belgians; Professor of Clinical Surgery, and Surgeon to University College Hospital. Third Edition. London: John Churchill and Sons, New Burlington-street. 1869.

ALTHOUGH we are not in the habit of reviewing books, earlier editions of which have been criticised in our columns, we will break through the rule with this volume, as it contains some new matter concerning urethrotomy and the treatment of stricture with mechanical dilators, a treatment which has excited much attention within the last few years in consequence of an increasing knowledge of Perrève's dilator in England and elsewhere. Moreover, the method Perrève follows in the use of the dilator, being different from that practised by Mr. Holt, and some evidence, not to be found in this volume, having recently transpired which tends to prove the superiority of fully forcing or rupturing a stricture, by a single use of the wedge, over its gradual dilatation by successive *séances*, as practised by Perrève, a somewhat extended account of the subject may not be unacceptable to the reader.

At a period like the present, when so many writers aim at ponderosity rather than at condensation, it is satisfactory to find this volume less in weight, by eighty pages, than its predecessor; many illustrative cases have disappeared from the work, books, Sir Henry observes, being far more readable when they give results "in the form of opinions, unencumbered by those guarantees which might naturally be expected from an author in the early part of his career."

The first 45 pages of this edition are occupied by a very excellent, and, we believe, an accurate description of the anatomy and physiology of the male urethra. They contain the most recent as well as the generally admitted views regarding the structure of that most useful passage, but, unfortunately, frequent seat of human suffering. If we may judge by our examinations of the canal, Sir Henry Thompson's account of it is so truthful that it may be safely recommended to the student for his guidance while investigating the ordinary and the histological anatomy of the male urethra.

The remaining portions of the book are devoted to the con-

sideration of the pathology of stricture; the causes of organic stricture; spasmodic and inflammatory urethral obstruction; the diagnosis and treatment of stricture; retention of urine depending on stricture; urinary abscess and urinary fistulæ; stricture of the female urethra; rules for examining the urine for surgical purposes; notes on specimens of stricture in some of the most important collections of the United Kingdom; and lastly, a table of cases.

We have often heard it stated that when there is a stricture of the spongy portion of the urethra, in front of the posterior inch (the part of the passage proved by *post mortem* examinations to be the ordinary seat of stricture), it is usual to find a second coarctation nearer the bladder. This may be the case, but it is not in accordance with our observation. There are in the Museum of the Royal College of Surgeons, Dublin, six preparations, and we possess another, in all of which the anterior portion of the urethra is strictured. Of these preparations, but one has a second stricture, which is near the bulb; possibly when there is a stricture anterior to the ordinary seat of contraction it may be exceptional to find a second in the latter situation; the assertion regarding the existence of the second or posterior stricture has probably been based upon observation at the bed-side; but in deciding a question of the third, *post mortem* facts are alone conclusive. We should like to see the point elucidated in the next edition of this work, for the interesting information collected by the author as to the multiplicity of strictures in the same urethra, although it bears upon, does not decide the question. There may be, for instance, a stricture in the ordinary situation, and a second behind the bulb, or two strictures in the last inch of the spongy portion of the canal. The question, however, is, when there is a stricture, say two inches from the orifice, is it the rule to find a second between it and the bladder?

With regard to the number of strictures in one urethra, Sir Henry Thompson tells that—

“John Hunter records an instance in which he met with *six* strictures in one urethra. Lallemand mentions one with *seven*; Colot saw one with *eight*; Ducamp says there are rarely more than two, but that he has seen *four* or *five*; Boyer thought *three* could exist together. A case is reported by Leroy D'Etiolles, in which he found *eleven*; but since this is sometimes quoted as if it were a *post mortem* observation it is necessary to state that this number rests only on the evidence afforded by the passage of an exploratory bougie (that is, a small gum-elastic sound, with an olive-shaped extremity, two or three sizes larger than that of the stem),

on the person of a *living* patient. The strictures, to use the author's words, were '*for the most part in the spongy portion, about two and a quarter lines distant from one another;*' a condition which would perhaps be better described as a series of irregular contractions, than by any statement of the exact number of strictures. Rokitsansky speaks of four, and does not record a higher number as having come under his own personal observation."—P. 51.

Voillemier^a mentions that in a few cases he was led to think there were a greater number than Leroy had described, cases in which the urethra was diseased and contracted almost its whole length. There was, properly speaking, but one long contraction; it, however, was more pronounced in certain points than in others. He has most often met with one stricture, sometimes two, and never more than three. His mode of accounting for the apparent multiplicity of strictures in a urethra contracted in a considerable portion of its length is confirmatory of the author's explanation of Leroy D'Etiolles' case.

Great pains have been taken by Sir Henry Thompson to collect evidence as to the most frequent seat of stricture in the male urethra. All the authorities quoted by him, with a single exception, nearly "agree in one particular, viz., that stricture is most commonly found at the sub-pubic curvature." "No doubt can exist as to the fact, which John Hunter observed, that organic stricture is almost invariably limited to that portion of the urethra which is anterior to the deep perineal fascia." A circumstance not to be wondered at when we recollect that the bulbous portion of the urethra is especially liable to become the seat of gonorrheal inflammation. Sir Henry Thompson's own observations lead him to coincide, "beyond all doubt," with the view "which assigns the first place, in relation of frequency of occurrence, to the stricture which occurs at the posterior part of the urethra." This is our experience also. The question, however, is one which can only be accurately decided, as he remarks, by identifying "the contraction with the anatomical regions of the urethra," and not by trusting to "measurements from the orifice only." In the excellent and most instructive Museum of the Royal College of Surgeons in Dublin, the statement of Sir Henry Thompson on this point, as Mr. Albert Walsh has shown, is amply confirmed by the preparations of stricture in it. And we may mention that shortly after we had been

^a *Traité des Maladies des Voies Urinaires. Maladies de L'Urèthre. P. 126.*

sent Sir Henry Thompson's work, we examined the preparations that have been added to the collection since the publication of Mr. Walsh's description, and found that in these, likewise, the spongy portion is alone affected; and that in the majority of the specimens the stricture is situated either at or near the bulb. No specimen of stricture, in the prostatic portion of the urethra, has been added to the College collection subsequent to the specimen described by Mr. Walsh, and alluded to by Sir Henry Thompson.

Voillemier, in speaking of the seat of urethral stricture, states that, as for himself he has derived his information from numerous autopsies, and that "the contractions may be seated in every point of the urethra, from the *meatus urinarius* even to the neck of the bladder inclusively. Their seat is ordinarily in relation with the cause which has produced them. Those of the *meatus urinarius* or of the navicular fossa, are often the consequence of syphilitic ulceration; those of the spongy portion succeed rather to a urethritis; finally, those of the membranous region are almost always the result of a traumatic lesion. We can understand why there should be numerous exceptions. I have myself," he adds, "described strictures of the anterior portion of the urethra, and of the prostatic, produced by urethritis; a chancre may develop itself tolerably far in the urethra, and external violence may injure all portions of the canal."* Syphilitic ulceration, we should observe, may engage even the bladder itself.

An apology for bestowing so much labour in determining the most frequent seat of organic stricture was not necessary from Sir Henry Thompson, the question bearing, as he remarks, so intimately upon its mechanical treatment.

Many writers on the surgical diseases of the urinary organs, the author among the number, seem to consider that extravasation of urine is invariably due to straining efforts acting upon a canal diseased behind the stricture. "Thus," he writes, "during one of those fits of retention already noticed as frequently supervening on permanent stricture, while the patient is vainly tasking his strength to the utmost to void his urine, not voluntarily, it may be remarked, for the painful and laborious efforts at straining in which the whole system appears to participate, are then, to a great extent, reflex and involuntary, of which the distended bladder is the exciting cause, sudden relief is afforded."

* Ibid, p. 127.

This, there can be no doubt, is the true history of urinary infiltration in the majority of cases in which it occurs, but that it may take place independent of straining, is evident from a case which will be found recorded in the November number of this Journal^a. Sometimes the urethra will give way at a considerable distance from the stricture, an occurrence which is remarkably exemplified by a specimen in the Museum of the Royal College of Surgeons, Dublin. In this specimen, which was presented to the College by the late Mr. Trant, the stricture, half an inch long and very close, is situated at the orifice; but the laceration is in the superior wall of the canal, corresponding to the junction of the membranous portion with the bulb; it appears as a longitudinal slit, about an inch in extent.^b The urethra gave way "during a desperate fit of straining to relieve the bladder."

There is no subject of more painful interest to the surgeon than the pathology of sudden death in patients under treatment for stricture. Sir Henry Thompson alludes to his having observed death to result, "on more than one occasion," from "suppression of urine, caused by "the introduction of an instrument larger than the patient has been accustomed to, in the hands of a surgeon who, from some accidental cause, has replaced the ordinary attendant, and who has unwittingly endeavoured to carry dilatation beyond the usual limit." We would add that death has occurred from catheterism also, independent of any such unwitting attempts. Should renal disease complicate stricture, the dangers of catheterism become seriously multiplied. "The rapidity with which death may occur," he remarks, "in patients who are the subjects of extensive chronic disease of the kidneys, from an apparently exceedingly trifling operation so caused, appears almost unaccountable." A *post mortem* examination fails to reveal any acute mischief, and, in the absence of such, the author falls back on the ordinary explanation of the so-called uremic symptoms, a theory to the acceptance of which, if this were the place for such a discussion, we could show there are grave objections.

The following judicious observations of the author on the practice of catheterism are so thoroughly practical, we are sure they will meet with the approbation of every sound surgeon:—

^a Page 466.

^b Descriptive Catalogue of the Preparations in the Museum of the Royal College of Surgeons in Ireland. By John Houston, M.D., M.R.I.A. Vol. ii., p. 465. F. b. 323.

"Whatever the obstruction it is never to be carried by storm. A patient, persevering, and unruffled spirit, with a light and dexterous hand, will work miracles in cases of difficult catheterism. All attempts at display, at brilliant manœuvring, at rapidity of execution, should ever be deemed wholly out of place, fraught with danger to the patient, and if so, surely calculated to redound, at some time or another, to the discredit of the operator."

Our experience of the superiority of the French catheter gauge over the English one enables us to confirm all that Sir Henry Thompson says in its favour. Were it not for the fine French bougies (the $\frac{1}{3}$ and $\frac{2}{3}$), as well as the larger half sizes, our initiatory dilatation of several very close strictures would have been unnecessarily prolonged. There is room for improvement, however, in the quality of some of the imported instruments. We have over and over again observed their liability to crack near the point, even when they have been but little used, and that they are particularly liable to do so just behind the olive or bulb when they are thus shaped.

We are glad to find that such eminent manufacturers as the Messrs. Weiss are now making all their instruments on the French scale.

Sir Henry Thompson is not an advocate for continuous dilatation in simple cases of stricture, believing that better and safer results are obtained by withdrawing the catheter or the bougie immediately after it has entered the bladder (as Luxmoore recommended) than by leaving it in the urethra from a few minutes to half an hour, as practised by some surgeons. We rarely act otherwise than Luxmoore did, with simple cases, even in those in which we complete the treatment with the dilator, having previously enlarged the stricture to No. 4 or No. 5 gauge.

The following judicious observations upon this point of practice are reproduced by the author from Luxmoore's book, published in 1809:—

"Our proceedings should be so cautiously conducted, that a bougie, once passed, should be continued for no greater length of time than the patient can easily bear; for it is the great fault of those who are engaged in the treatment of these affections that, when a bougie is once lodged in the urethra, they are of opinion that it cannot be too long retained; not considering that the introduction of a foreign stimulus too long continued, or too often repeated, must, in a certain degree, excite morbid as well as

healthy actions; and, if the former prevail, which will be the case should inflammation be produced, fresh coagulable lymph will be deposited, and a new organization take place. Instead, then, of twenty minutes or half an hour (which is the usual time for the application of each bougie), I seldom continue it longer than a minute or two at each successive introduction. Having once passed a bougie, in this slow and cautious manner, through the stricture, I seldom consider this instrument any longer necessary."

The free application of oil to the urethra to facilitate the passing of the catheter or the bougie has been adopted by the author. He recommends the oil to be injected into the urethra with a common glass syringe; but Perrève, who injects the canal that his dilators may more easily glide through very narrow strictures, uses a female catheter, six inches in length, open at its vesical end, and a small injecting syringe. The catheter is passed to the stricture, and the injection cautiously made.^a We have tried the plan, and fancied that it was of some assistance in a few cases.

The condemnation of gutta-percha bougies in this volume will have some influence, we hope, in causing their disuse. As the author observes, they are liable to "elongate considerably in the act of withdrawal," and "unless great care be taken, a portion may be left in the urethra."

So brittle does gutta-percha become after it has been kept any length of time, that we think with him "that no instrument made of it should ever be used for the urethra."

The sea-tangle bougie also is a very unsafe instrument, and should be carefully tested before use. When once wetted, dried, and laid aside for a few months it will frequently break like glass. Besides, the property which the sea-tangle bougie possesses, of absorbing liquid, swelling and dilating a stricture rapidly, provided it has entered it, renders the employment of this kind of bougie fraught with danger if the instrument clears the stricture; for, owing to the resistance of the latter, the bougie can swell more behind it than in it, and if not removed at the proper time, it may swell so much behind the stricture as to necessitate the cutting open of the urethra for its removal. This property of swelling when moistened belongs also to the cat-gut bougie, of which every surgeon is aware. It, likewise, may enlarge to such a calibre behind a stricture that its removal without operation becomes an

^a *Traité des Rétrécissements Organiques de L'urètre.* Par Victor Perrève. A Paris, 1847, p. 158.

impossibility. This is not mere theorizing, for we have been informed by a gentleman, upon whose accuracy every reliance can be placed, that he has seen the urethra opened for the removal of a cat-gut bougie which had become so swollen behind a stricture that it could not be withdrawn.

Having fully described the details of the treatment by dilatation, the author proceeds to give an account of the various instruments which have been devised for the cure of stricture by expansion and by rupture, and here we were glad to find justice done to M. Perrève, the omission of whose name even from the last edition of Mr. Holt's book on the immediate treatment of stricture has exposed that gentleman to some not undeserved criticism. From M. Perrève's *Traité des Rétrécissements* (Paris, 1847) Sir Henry copies an illustration of the dilator now used by that distinguished surgeon. We were surprised, however, to find him referring us to M. Perrève's Treatise for a description of an instrument formed of two blades united at the extremity, which, by means of a screw, can be separated from each other after they have passed into the stricture, though Sir Henry correctly attributes this instrument to M. Perrève. No account of it is to be found in the *Traité des Rétrécissements*. On this subject, indeed, the full amount of our debt to the inventive genius of the illustrious Frenchman is so little known, and the subject itself is of such practical importance, that we cannot do better than give our readers a very brief account of the progress of invention in urethral dilators. Nearly sixty years ago Mr. Luxmoore employed an instrument with four blades, which were made to expand by means of a screw; subsequently Leroy D'Etiolles constructed a somewhat similar one, but neither of these appears to have answered the purpose for which it was intended. Between thirty and forty years ago, however, M. Perrève directed his attention to the subject, and in 1837 obtained a patent of invention for the instrument (Fig. 1). This was constructed on the screw and knee-lever principle. Shortly after Perrève had obtained the patent, Civiale adopted its principle in the construction of one of his dilators (Fig. 2), with this difference, that in the latter the blades do not separate parallel to each other, the greatest amount of separation taking place either at or near the stricture. In this dilator the knee-levers are raised as they are in Perrève's screw instruments (Figs. 5 and 6). Still adopting the screw and knee-lever principle, M. Perrève subsequently constructed the dilators of which wood-cuts are given in Figs. 3 and 5. To none

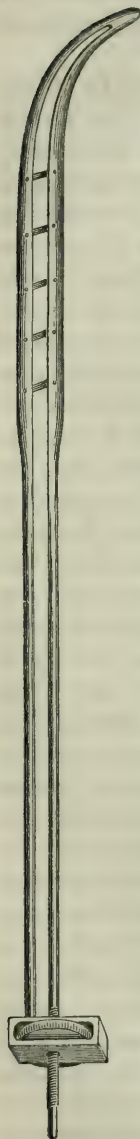


Fig. 1.—M. Perrève's first screw and knee-lever dilator. In this instrument the dilatation is limited to the contracted part of the urethra. The little bars or levers are raised by the same mechanism as in Figs. 2 and 3. Copied from the Invention Patent of 13th July, 1837.

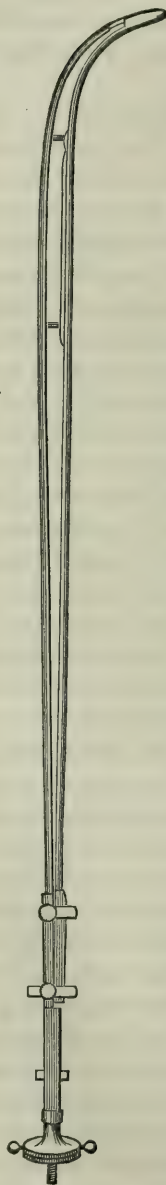


Fig. 2.—The dilator illustrated by Civiale. His intention in its construction was, to limit dilatation to the contracted part of the urethra. Perrève effected the same object by thickening or deepening the blades where they correspond to the little bars (Fig. 1) Civiale describes in his "*Troisième lettre sur la lithotritie*," a dilator in which the blades are separated by a lattice arrangement, but he supplanted it by Fig. 2. There is no allusion to either of these instruments in the first edition of his *Traité Pratique Sur Les Maladies Des Organes Génito Urinaires*, published in 1837—the year Perrève patented Fig. 1.

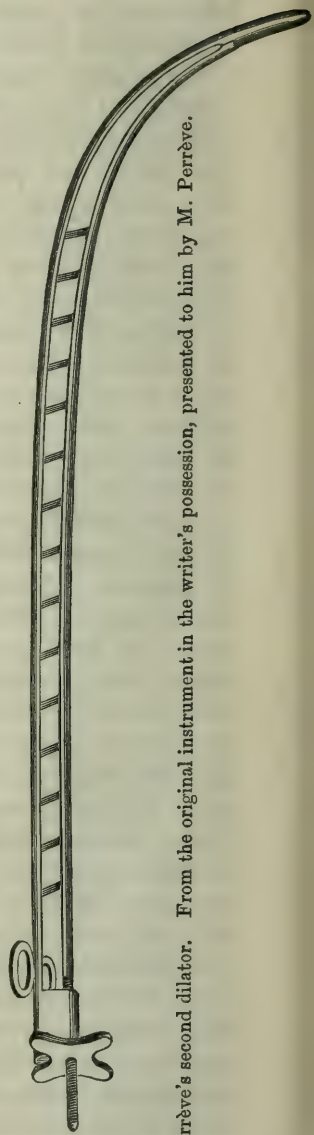


Fig. 3.—M. Perrève's second dilator. From the original instrument in the writer's possession, presented to him by M. Perrève.

of these is allusion made in his work published in 1847; he had then abandoned this kind of dilator altogether, because, as he states in a letter to us, he found "the mechanical system upon which it is constructed does not permit of giving its dimensions sufficiently small to penetrate close strictures without its being rendered too fragile, and, moreover, because the movement being effected by a screw, the operator does not experience, during the divarication of the blades, *the sensation of a resistance overcome.*" Immediately after Fig. 3 we place Fig. 4, attributed by Voillemier both to Michélena and Rigaud, the former having described the instrument in 1847 (*Thèses de Paris*), and the latter in 1849 (*Gaz. de Strasb.*); in other words, after M. Perrève had invented, tried, and abandoned similar instruments. For comparison with Fig. 5 we have placed the instrument (Fig. 6) described in Weiss' Catalogue as Lyons' dilator.

After giving up the screw and knee-levers the ingenious Frenchman invented the instrument (Fig. 7), the only mechanical dilator, which he describes in his Treatise, and of which a wood-cut is given in the work before us.

Fig. 8 is the instrument the author himself uses. Although it contains many improvements, not to be found in the screw instruments to which we have alluded, it is worked on the knee-lever principle, and, like the dilator illustrated by Civiale, the separation of the blades is greatest at about the junction of its middle with its vesical third. The little bar is attached by one end to a rod, by the sliding of which it is brought to a right angle with the blades, as in Figs. 2, 5, and 6. Its action upon a stricture is purely eccentric, provided the knee-lever or short bar corresponds to the stricture. Lastly, in Fig. 9 we give an illustration of the dilator invented by the writer of this notice, fully described in the *Dublin Quarterly Journal* for August, 1868. Sir Henry Thompson has been using this instrument. "Dr. Richardson, of Dublin," he writes, "has suggested a modification of Perrève's instrument which cannot be passed over here, since there is no doubt its action is more perfect than that of the original. It is applied in the same manner as by Mr. Holt. I have used it several times, always employing a full calibre, about 15, English scale, which I think essential to good results. After the operation I prefer to tie in a gum catheter for twenty-four hours, keeping the patient in bed for that time."

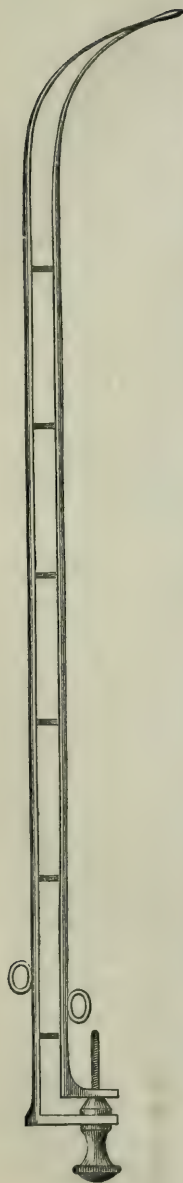


Fig. 4.—The dilator attributed by Voillemier to Michélna and to Rigaud. A similar instrument is figured in Maw's catalogue as Lyons' dilator. The little bars are raised by the same mechanism as in Fig. 3, M. Perrève's second dilator.



Fig. 5.—M. Perrève's third dilator. From the original instrument presented to the writer by M. Perrève.

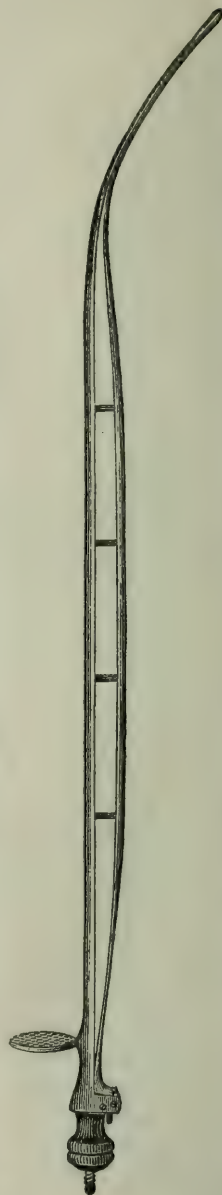


Fig. 6.—The dilator called in Weiss' catalogue Lyons' dilator. The little bars are raised by the same mechanism as in Fig. 5, M. Perrève's third dilator, namely, by the sliding of a rod to which one end of each is attached; the rod moves on the lower blade, and is acted upon by a screw arrangement at the handle.



Fig. 7.—M. Perrève's fourth dilator (on the wedge principle), or the one now used by him. From the instrument presented by M. Perrève to the writer : being the original instrument from which the outline drawing, Fig. H, pl. 3 of M. Perrève's *Traité des Rétrécissements* was taken.



Fig. 8.—Sir Henry Thompson's dilator. Drawn from the instrument, on the same scale as the other woodcuts.

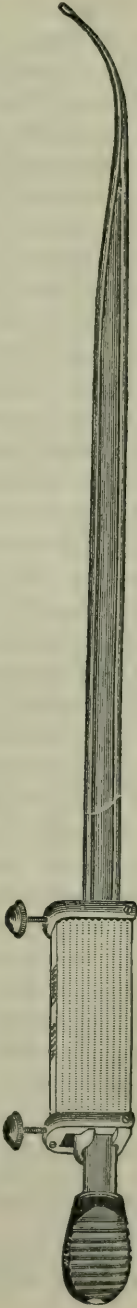


Fig. 9.—Richardson's tunnelled-handled double-bladed dilator.

The procedure for which these various instruments have been devised—namely, the forcible rupture of stricture—seems to have very strongly commended itself to Sir Henry Thompson. “This plan,” he says, “has now been largely used by the profession, and several hundreds of cases have been operated upon; and with results which warrant the formation of conclusions as to its value. It possesses the merit of simplicity, for provided the first instrument (the closed blades) a guide, is safely passed through the stricture into the bladder, the introduction of the large tube—the conical wedge, Fig. 7, or the dove-tailed wedge of Fig. 9)—which effects the rupture is only a matter of force. No other operation for stricture is so easily accomplished, or is so unlikely to fail in the performance. To divide a stricture by any method of incision, requires much more knowledge, practice, and tact; while it is, I believe, a more perfect proceeding in its results for really hard, old, and contractile strictures than rupture.”

On this subject we may mention that a few cases have been lately published which induce us to consider that the course Perrève follows—the multiplication of the *séances*, although so eminently successful when practised by him, not having proved fatal, he states in his work, in a single case out of 150 to 200, nor in any case he treated, subsequent to its publication,^a may not, after all, be the most judicious. This evidence bears upon another question—namely, the immediate action of the dilator upon the stricture—*i.e.*, whether it is split, or merely dilated. Perrève himself is of opinion that the rupture of the stricture is impossible when the dilator is used according to his method^b; but Civiale, some years ago, expressed himself differently,^c maintaining that the operation produced an actual laceration of the tissues; and even the forcible use of a bougie, in John Hunter's opinion, tore the stricture. On this point, however, there is no doubt Civiale spoke too decidedly. Dr. Millar, of Edinburgh, has shown by a *post mortem* examination that the use of the dilator does not necessarily produce rupture of the mucous membrane; and in this city similar evidence was lately produced by Dr. Robert M'Donnell. A patient on whom he had operated, and in whom not a single bad symptom had been produced by the operation, was carried off by cholera, then

^a Medical Press and Circular, April 7th, 1869.

^b *Ibid.* P. 182.

^c *Traité Pratique sur les Maladies Des Organes Génito-urinaires.* Par Le Docteur Civiale. Troisième Edition. Tome Premier. Paris, 1858. P. 267.

epidemic, and on an examination of the urethra, no rupture could be discovered. On the other hand, his colleague, Mr. Edward Hamilton, has recorded an instance in which an opportunity having occurred of examining the body of a man whose stricture had been dilated by the instrument, usually known as Mr. Holt's, a laceration of the mucous membrane was discovered. A similar result occurred in the practice of M. Bérard; and in one of two cases under the care of M. Cusco, in the Lariboisière. On the main question—the comparative safety of dilating the stricture at a single sitting, or by repeated applications of the dilator, M. Cusco's cases bear most strongly.

The subject of the first case was a man thirty-five years of age, who entered the Lariboisière Hospital on the 15th of May, 1863, to be treated for stricture. The *first* dilatation was practised with Perrève's dilator, *without causing any accidents*. The 24th of May a second dilatation. The patient was placed in a bath, and a little time afterwards was seized with a rigor. He eventually died.

The second case was that of a man fifty years of age. The *first* dilatation was made on the same day as that of the preceding patient, and *was not followed by any accident*. Submitted to a second dilatation on the 23rd of May; he was then placed in a bath. He, also, was seized with a rigor. He subsequently died. At the autopsy, the kidneys were found to be very congested, and the bladder hypertrophied and contracted. The urethra was narrow almost its whole length; there were some ecchymosis, and a laceration of the mucous membrane corresponding to the bulb. It is probable that in this case the kidneys were diseased at the time of the operation.

To our mind these cases, notwithstanding the non-fatality of M. Perrève's own operations, even when the patient has submitted to several *séances*, are conclusive against the multiplication of these *séances*. We think, therefore, that as a rule the stricture should be fully overcome by a single use of the wedge—for, as Voillemier^a remarks, we have then only to deal with either indurated or cicatricial tissues, the vitality of which is obscure; whereas later, these tissues, when they have been forced by the instrument, inflame and acquire very great sensibility. If this be a correct explanation, we can have no difficulty in understanding why the operation known as the "immediate method," and which has been

^a Voillemier. Ibid.

practised in so many hundreds of cases, should have a mortality so slight as to leave us undecided—whether the stricture is split, or merely dilated, in the majority of persons submitted to the operation. On the other hand, it is Sir Henry Thompson's opinion that during the treatment of stricture by bougies—even when carried out in the very gradual manner he recommends—congestion of the parts follows each introduction of the instrument; the process is necessarily a tedious one, requiring from thirty to seventy days, and by no means infrequently gives rise to urethral fever, and other accidents. Continuous catheterism (*à demeure*) is liable to produce, as Voillemier has shown, the most serious inflammatory changes, purulent infiltration, and ulceration, in the corpus spongiosum, and other grave complications. Now, such being the case, we feel almost compelled to view the “immediate method” as the procedure which should be adopted as the ordinary practice in the majority of cases of stricture of the urethra.

Having discussed the use of caustics in stricture—which, we need hardly say, finds no favour with the author, he proceeds to consider the treatment by internal division, and gives an account of various urethrotomes. He has apparently committed an oversight regarding the estimation in which M. Reybard's urethrotome was held by the judges for deciding upon the Argenteuil prize. He states, that “the instrument and method of M. Reybard attracted much attention some time ago, having gained the Argenteuil prize of 1851, besides being the result of much laboured study of the subject.” In truth, the instrument did not gain the prize. It would, indeed, have been strange if so terrible a urethrotome as Reybard's had been decreed a prize by a commission, of which M. Ricord was a member. “The Academy of Medicine,” writes Nélaton, “has crowned a work in which this mode of treatment is extolled; but see how one of the members of the commission lately expressed himself, M. Ricord, before the *Société de Chirurgie*. ‘M. Ricord has signed the report which gave the prize to M. Reybard. But it was not for his frightful instruments—the memoir contained solid reflections upon stricture, very original ideas—it is this which has decided it.’”^a

Civiale's urethrotome, which, we have no doubt, has been used with that manual dexterity for which Sir Henry Thompson is so justly celebrated, has not, in the hands of other surgeons, been so

^a Bulletin de La Société de Chirurgie, 1854-1855, t. v. P. 409. Also Nélaton. Ibid.

facile in working. M. Robert found that it cuts the tissues badly; so that in order to divide the stricture, it is often necessary to use very strong traction upon the instrument, which displaces the diseased tissues, folds the mucous membrane, and exposes the latter to be cut irregularly, and to a great extent.^a

Voillemier, also, who has frequently used this urethrotome, says, that notwithstanding the care he has taken to develop the blade gently, at the same time that he drew the instrument to himself, in order to make it saw, he was obliged to use a tolerably strong force to cut the contraction.^b M. Caudmont's modification of this urethrotome seems to be a great improvement on the original, particularly as the back of the blade is supported much more solidly than it is in Civiale's urethrotome—a most important quality when the stricture is very resisting.

In consequence of the slenderness of the grooved steel conducting rod of Maisonneuve's urethrotome, it is liable to break during the attempt to pass it through the stricture. M. Icard asserts that he has seen it do so, in consequence of the force employed to engage it in a stricture.^c This, however, should be attributed to the manner of using the instrument rather than to its imperfection.

"The true defect of this urethrotome," Voillemier observes, "is, that its blade is entirely exposed, in place of being concealed in the canula, and there is nothing to protect the urethra from its cutting edge." "M. Maisonneuve believed that it would suffice for the blade of the urethrotome to be blunt at its summit, for the sound parts of the canal to glide from before its cutting edge—whilst the indurated and resisting tissues are alone divided. In this he is deceived. Experiments upon the dead body, and observations made at the bedside, leaves me no room for doubt on this point."

"In the hospital *Beaujon*, one of our colleagues performed urethrotomy with Maisonneuve's instrument. The patient died; and, at the autopsy, the canal was found to be cut its whole length. By a curious chance, the contracted part was that which had been the least deeply cut."^d

Maisonneuve has, we understand, altered the shape of the cutting blade; but we are still of opinion that it is a dangerous instrument.

^a *Lettres sur l'urethrotomie*, 1855.

^b *Ibid.* P. 260.

^c *Des Rétrécis. de L'urèthre.* 1858. P. 30. Also Voillemier. *Ibid.* P. 267.

^d *Ibid.*

Voillemier has modified this urethrotome, having added to it a plate for concealing the blade, with some other alterations.

We were gratified to find that the author has not omitted to mention the late Abraham Colles' mode of dealing with those remarkably obstinate constrictions engaging the external meatus, which do not yield to dilatation, and frequently relapse after incision. Ricord, of Paris, and Weber, of Bonn, Sir Henry Thompson states in a note, practise Mr. Colles' operation, or a slight modification thereof; but the principle is the same in both.

The following are Sir Henry Thompson's conclusions regarding internal urethrotomy:—

“It is indicated in almost all strictures affecting the external meatus of the urethra; and for many cases of stricture situated about the middle of the spongy portion, for which dilatation has proved unsuccessful, it is the most efficient treatment existing.

“It is useful, also, in some few cases of stricture situated at the bulbous portion, which are not relievable by dilatation; a single incision is not deep, being free from danger, and frequently rendering the stricture perfectly amenable to dilatation afterwards. Lastly—it is so in those rare cases in which the urethra is narrowed and indurated at many points, or throughout a great portion of its course, dilatation having been found inefficient. But in the two latter classes, the treatment by rupture is, perhaps, as useful in most cases, and much easier to perform.”

As the author does not give us the percentage of cures, &c., which have resulted from his internal urethrotomy operations, it may not be uninteresting to mention the opinions and experience of a recent very painstaking writer, regarding this operation.

“I have read,” says Voillemier, “with the greatest care most of the observations which have been published, taking into account those only which appeared to me irreproachable. I have examined my own facts with the greatest rigour, and I have arrived at this result—that the number of deaths is nearly one in thirty. This number is, without doubt, considerable, when we apply urethrotomy indiscriminately to all contractions; it is small, on the contrary, if we practice the operation only for grave strictures, which are rebellious to dilatation. But that the mortality is a little more, or a little less high, has but a secondary importance with the surgeon; who will have recourse to urethrotomy only in cases in which he will judge it absolutely necessary.”

“From 1860 to 1864, the period of time for which I have very

exact dates, I have met with 14 relapses in patients who had been operated upon by other surgeons." "If now I consult my own facts see the results I have obtained—out of 45 patients I have operated upon, two died—of 43, I have met but 15 again at the end of the year 1866."

"In 9, a bougie of 2 *millimètres* $\frac{2}{3}$ to 3 *millimètres* is passed with difficulty. Two of the patients did not continue the use of the bougies, and five had used them only three or four months, one very irregularly."

"In five I have been able to introduce only a bougie of 3 *millimètres* to 3 *millimètres* $\frac{2}{3}$. They had all used bougies during a few months; but could not in this regard give precise information."

"In one micturition is performed tolerably well. I have been able to practise catheterism with a bougie of 4 *millimètres* $\frac{1}{3}$. This patient, an intelligent workman, has not ceased to sound himself, although at very irregular intervals."

"In *résumé*—urethrotomy, of which, I believe, abuse has been made, and the success exaggerated—cannot be considered as a general method of the treatment of contractions. It is but a palliative means, incapable of procuring a recovery of long duration, and still less a radical recovery. But it may come to the assistance of dilatation, and, completed itself by this last method, it is of incontestable utility in a small number of cases." *

The chapter on external urethrotomy will be read with great interest; the history of the operation is traced from its earliest mention down to the present time, and the various misconceptions regarding it explained. The author's opinion, founded on his extensive experience, is quite in accordance with our own. "The cases," he says, "for which this operation may now be reserved are mainly those in which large, numerous, or obstinate perineal fistulæ co-exist with old or obstinate strictures. When other treatment has failed, and the fistulæ refuse to heal—even although the patient has withdrawn his urine for some weeks entirely by the catheter—no proceeding, perhaps, offers so good a chance of cure as this. It is for such cases I reserve it now; and, as these are extremely rare, it is seldom necessary to have resort to it. Since the last edition of this work I have performed it only four times—the last case being in 1868. With nine previously published cases, my entire personal experience amounts to thirteen cases of division of stricture from

* *Ibid.* P. 309.

the external surface upon a slender grooved staff; the old operation without a guide, as before said, I have never had occasion to perform. Not one was fatal; and the results have been, on the whole, satisfactory, and warranted my appeal to the proceeding as a last resource in the worst form of disease. For such, my views of its value remain unaltered, although there are other means, particularly that already considered under the title of 'rupture'—which, as more easy of performance, and involving less risk, should be preferred for the great majority of cases not amenable to dilatation."

Voillemier expresses himself in a somewhat similar way—"In all these cases," he writes, "I would, perhaps, be of opinion with Mr. Syme, if we had not at our disposal any other resource than that of incising the urethra from without. But we have internal urethrotomy, and especially divulsion ('rupture'), which, without being so dangerous as external urethrotomy, will give the same results."^a

In the chapter on retention of urine, Sir Henry Thompson, after giving a very concise account of, and some thoroughly practical rules regarding retention, mentions curious facts concerning the relative frequency of the operation of tapping the bladder in some of the London hospitals.

"Much difference of opinion exists," he says, "respecting any such operation, and much variety in practice. It is notorious that there are hospital surgeons of extensive experience who have never performed any such operation for the relief of retention at all. Others are equally well known to have done it ten, twenty, and even fifty times. For example—in St. George's Hospital there has been no case of operation during twenty-five years. In Guy's Hospital, on the other hand, as many as thirty-six punctures through the rectum have taken place during the past six years.^b Is this dissimilarity in practice attributable to any difference in the class and character of patients who frequent the two hospitals in question? That a difference does exist there is no doubt. The neighbourhood of the borough hospitals supplies a vast number of neglected patients of the most dissolute and intemperate habits, and it cannot be doubted but that the population of the banks of the Thames—including as it does a large proportion of sailors—affords more numerous and more aggravated examples of the disease than that which inhabits the western districts of the metropolis. But an

^a Voillemier. *Ibid.*

^b The date of these remarks is 1852.

inference must not be too hastily drawn from this fact. What is the experience of the officers of the London Hospital?—an institution the patients of which belong usually to a class which may, at least, vie with those of Guy's in point of degradation of character and habits. Here an operation for the relief of retention has not occurred twelve times in a period of as many years. Mr. Liston states, that the operation of puncturing the bladder by any method was not performed in the Royal Infirmary of Edinburgh during the whole period of his official connexion with it; nor during that of his association with University College Hospital. In reference to that, as well as to the method of relieving retention by perineal section, he says—‘the cases requiring either proceeding will be rare indeed, if the mode of using a catheter be properly understood.’”

In the enumeration of the operative procedures for the relief of retention—when the surgeon who may fail to pass an instrument along the urethra to the bladder considers an operation necessary—the author does not mention the sub-pubic operation claimed by Voillemier as his invention. This omission is probably owing to Sir Henry Thompson not having had the opportunity of seeing Voillemier's work, the publication of which preceded this edition by only a few months. Voillemier punctures the bladder with a trochar, a little less curved than that of Côme's. It is passed between the pubis and penis in its course to the bladder. But as it may perforate the dorsal vein of the penis, or the anastomosing veins with which it communicates, we apprehend this operation will not meet with much favour. It is only right, however, to say that the only person upon whom Voillemier performed this operation recovered.

Speaking of the operation through the rectum, the author remarks, that “the chief objections which have been raised against it are the averred liability to the occurrence of abscess between the rectum and the bladder as an after result; the persistence of fistulous opening there; the infliction of injury upon the seminal vesicles leading to inflammation of these and the neighbouring parts, including the testicle; and the danger of perforating the peritoneum with the trochar, and thus setting up inflammation of that membrane. All these results have, undoubtedly, been met. One or two instances, perhaps, of each might have been recorded here. I have even known suppuration of the testicle from inflammation to be thus caused.” Again:—“But Mr. Cock's practice seems to have been remarkably free from these complications; and it is but fair to

believe that the dangers of the rectal operation have been over-rated. Having carefully examined the reports of forty cases, as given by that gentleman in the paper alluded to, I find seven or eight deaths following the operation; but no evidence that these were caused by it. In five cases the patients had suffered from stricture for many years, and in all advanced renal disease existed. In *none* does it appear to have arisen from any of the causes hitherto alleged to be sources of danger. So readily do these punctures heal, that it has been suggested by a surgeon, who has himself tapped the bladder several times, and has, therefore, had some experience of the results, in certain rare cases in which considerable irritation of the bladder is kept up by the continued presence of the canula, to make a fresh puncture every day, in order to avoid it. Whatever may be thought of the proposition, the facts I have stated are an indication that there is little disposition manifested by these openings to take on a fistulous character. It should be added, that Mr. Cock states that he has never met with an instance of this—at all events, not of its persistence after the permeability of the urethra had been restored.”

For our own part, we think, that when the supra-pubic operation is practicable, and an operation is *really* called for, it is to be preferred to any other mode of tapping the bladder. It is, comparatively speaking, free from risk, and the patient is saved the annoyance caused by the presence of the canula in the rectum, which sometimes has to be removed even before the stricture has become amenable to the bougie. Since this notice was written, M. Dieulafoy's aspirator (syringe) has been recommended for the evacuation of urinary collections.

Although it would appear that the recto-vesical opening has but little tendency to remain fistulous—we believe that it is more liable to do so than the supra-pubic one. But should it become permanently fistulous, to what misery will it not lead? Bonn, for example, has reported the history of a case, in which the opening remained fistulous for the remainder of the patient's life—a period of ten years. Every time the urine entered the rectum it caused an urgent desire to go to stool; and if this was not satisfied at once, involuntary defecation took place.

“To the serious accidents,” Voillemier observes, “that may complicate the recto-vesical puncture, must be added the *ennui* inseparable from it; the irritation produced by the continued presence of a foreign body in the rectum; the impossibility of sitting

or of walking; and, finally, the extreme difficulty experienced in the act of defecation.”^a

The puncture through the symphysis pubis proposed by Dr. Brander, of Jersey, and practised with success by himself and by other surgeons, has been tried but once by Sir Henry Thompson, and was a failure; no urine flowed, and he had to open the bladder above the pubis.

The author concludes the chapter on retention of urine from stricture with the following sound reflections:—

“But never be it forgotten, that every individual case offers a problem by itself, for the solution of which no rules can be positively pre-determined, while some cases there are which can be brought under no category, and in which the surgeon must exercise his own independent judgment, and rely upon his own resources. This assertion is a mere truism after all, applicable to the practice of surgery in all its branches. Although in none is it more desirable to keep it in mind than when called upon to meet the varied exigencies which, in complaints of the urinary organs, are apt to arise.”

The subjects of extravasation of urine, urinary abscess, and urinary fistulæ, stricture of the female urethra, and urethral growths are admirably discussed by Sir Henry Thompson; and in the appendix we have useful information regarding the examination of the urine.

Although the book is not large, it requires but little penetration to see what a vast amount of research must have been expended in collecting the historical materials for it.

As a compilation it could scarcely be surpassed; but as a practical treatise it will be some time, if we are not much mistaken, before it is equalled. No surgeon who undertakes the treatment of urethral stricture, and the various complications to which that distressing condition gives rise, should fail to make himself thoroughly familiar with its admirable precepts.

B. WILLS RICHARDSON.

A New and Successful Mode of Treating Certain Forms of Cancer.

By ALEXANDER MARSDEN, M.D.

So far as we can judge, Dr. Marsden's method of treating cancer contains nothing essentially new, and we doubt much if it be likely to prove more successful than other plans from time to time

^a Ibid.

suggested as cures for a hitherto irremediable disease. The author introduces the reader to his views by stating that cancer was an almost unrecognized disorder, indifferently understood by the bulk of the profession, until the Cancer Hospital of London was established some seventeen years ago. "To this institution" (it is scarcely necessary to mention), the writer adds, "it has been my good fortune to belong since its foundation." He has had, in consequence, opportunities open to few of observing every peculiarity of cancer amongst hundreds of cases collected together from all parts of the world. It is not his wish to discuss the general treatment of cancer so much as to advocate a method of treatment the result of many years' careful observation.

The author first gives his classification of the disease, and then details some valuable and interesting statistics derived from the registers of the Cancer Hospital. The table shows the relative frequency of the different forms of cancer in different parts of the body in the male and female. For instance, scirrhus cancer preponderates in the female breast; epithelial chiefly affects the male in the lip. Of 1,467 persons affected with epithelial disease 1,022 were males. Of 445 cases in which females suffered from epithelial cancer, the lip was affected in only five instances. The author then describes the peculiarities of the different varieties of cancer. In speaking of lupus, he gives herpes as one of the synonyms, a disorder it in no respect resembles.

Dr. Marsden then proceeds briefly to notice the multitude of remedies proposed from time to time for the treatment of cancer. In the course of these observations he truly remarks :—

"In the treatment of this disease, I cannot lay too much stress on the importance of being able, at an early period, to determine between malignant tumours and non-malignant; for if the patient place herself under the care of a practitioner unable to do this, it is obvious that she labours under a great disadvantage, as his treatment must not only be guess-work, but may, and in all probability will, result in great damage; perhaps converting a simple case into one of most serious importance. There is more mischief done by the application of strong and stimulating ointments, liniments, &c., to incipient cancers (these being sometimes mistaken for harmless tumours) than can well be imagined, even by surgeons taking a high place in their profession."

He then puts the questions, and gives the answers contained in the following extract :—

“Well, then, surgeons of the Cancer Hospital, out of this vast field what have you discovered? Have you found the specific for cancer? Have you rendered the disease no longer terrible? Alas! to the two latter of these questions I candidly answer, No; but to the first I can with pleasure and sincerity say, *Much*. We have discovered how, with almost unerring certainty, to distinguish at the patient's side true cancer from false. We have discovered a better method of treating true cancers, a method certainly resulting in longer life, and far less discomfort to the patient; in many cases we have discovered means of perfectly arresting the disease, and many more have been sent out of our hands, so far as time can show, cured.”

The means to which Dr. Marsden attaches such importance is the treatment by arsenical mucilage, which he says is applicable to all forms of cancer, except colloid cancer, when not of greater extent than four inches square. The treatment by the knife he does not discuss, but admits that it is oftentimes the most humane and efficient means at our command.

The arsenical paste used by Dr. Marsden is composed of two parts of arsenious acid and one of mucilage, and may be applied to cancers situated on any part of the body, except the mouth or nose. He does not recommend it for deeply situated cancer, but for those on or near the surface this paste is the most safe, painless, and certain remedy known to the author. The paste is not to be applied to more than one square inch of surface at one time. The *modus operandi* is, that a slough is formed, and spontaneous evolution of the cancer, by the opening thus produced, follows. One or more applications of the caustic paste is necessary to secure the desirable result. On page 64 the writer states the advantages of the arsenical paste, which are certainly, as used by him, very remarkable. The action of the caustic in Dr. Marsden's hands is very different, we believe, from what generally obtains in the practice of most surgeons, and by no ordinarily adopted method of treatment would epithelial cancer be permanently cured in nine cases out of ten, as Dr. Marsden asserts, he is able to do by his plan. One patient had the entire lower lip and chin affected with the disease. To Dr. Marsden's surprise he got quite well; all the disease was removed, and the lip assumed an almost natural appearance.

“One of the most pleasing and wonderful phenomena connected with the mucilage is the extraordinary power of election it appears to possess; for if put on with only ordinary care, the cancer alone is attacked, the

healthy structures remaining untouched, and the disease ultimately rolling out of a perfectly healthy wound.

“This treatment I have used with equal success in cancer on the lip, face, head, arm, hand, abdomen, breast, penis, testicle, labium, scrotum, and foot. I have never seen any bad results from its use, except in one case, and in this the evil was temporary only, and occurred in one of our earlier cases some years since. At the same time I must caution those who are inexperienced in its use that it is a dangerous remedy in unskilful hands, and requires constant watching; neither can it be used, as I before stated, to cancerous surfaces of greater extent than four square inches, and then only a small portion must be attacked.”

A number of illustrative cases and formulæ of medicines, which will be found useful in the treatment of cancer, conclude the volume. We can only say for our own parts, in terminating this notice, that we cannot entertain the same faith in the efficiency of arsenical mucilage for the cure of cancer that Dr. Marsden does. Caustics properly employed are frequently used for the removal of cancerous growths, and more often on the Continent than at home. It is a question yet to be decided whether, after treatment with the knife or caustic, the return of the cancerous deposit is the longer protracted. Be that as it may, the pretensions set forth by Dr. Marsden in favour of arsenical mucilage are utterly extravagant, and of his book it may, we think, be said, to quote the author's own words, when speaking of some other literary productions, “that if suddenly swept out of existence nothing would be lost to posterity.”

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1. *Renal Diseases: A Clinical Guide to their Diagnosis and Treatment.* By W. R. BASHAM, M.D., Fellow of the Royal College of Physicians; Senior Physician to the Westminster Hospital. London: John Churchill and Sons. 1870.
 2. *A Guide to the Examination of the Urine: intended chiefly for Clinical Clerks and Students.* By J. WICKHAM LEGG, M.D., Lond., &c. Second Ed. London: H. K. Lewis.

DR. BASHAM'S work is a compendious account of renal affections, intended for students and for junior practitioners. It is the work of a physician thoroughly conversant with his subject, and it deals with it in a concise but by no means in a superficial manner.

The arrangement adopted by Dr. Basham is far from perfect. He divides renal diseases into two great groups, under the headings

of Nephritis and Chronic Nephritis—the former comprising all diseases marked by symptoms more or less of an inflammatory character, and accompanied by bloody, albuminous, or purulent urine, the urinary sediment often exhibiting specific microscopic objects. The second group includes the different forms of chronic Bright's disease.

In the first group are collected together, along with acute Bright's disease, maladies as widely different as the tubercular and cancerous kidney. This mode of dealing with the classification of renal diseases has certainly the merit of simplicity, but it labours under the obvious defect of grouping together maladies which present extreme nosological differences; and although the author guards himself against being supposed to indicate that cancer of the kidney, for example, is necessarily or even frequently accompanied by inflammation, we cannot but think that it is highly undesirable that a classification should be adopted which entails the necessity of such explanations.

The work will, however, be found to be one of real utility, giving a clear and succinct account of the best and most recent pathological view on the subject of renal changes, with an excellent summary of the diagnosis and treatment of the different morbid conditions.

We would specially mention an admirable account of the physical, chemical, and morphological characters of the urine, which forms the closing chapter of the book.

Dr. Wickham Legg's little book is well calculated to assist the student in the acquisition of an exact and methodical manner of carrying on the examination of the urine. It has already reached a second edition, and we have no doubt will prove a popular and useful guide to students.

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1. *The Neuroses of the Skin: their Pathology and Treatment.* By HOWARD F. DAMON, A.M., M.D. Philadelphia: J. B. Lippincott & Co. 1868. Pp. 114.
 2. *On Neurotic Cutaneous Diseases, including Erythema.* By N. S. PURDON, M.D. London: H. K. Lewis. 1869. Pp. 116.
 3. *Lectures on Ekzema and Ekzematous Affections.* By ERASMUS WILSON, F.R.S., F.R.C.S. London: John Churchill and Sons. 1870. 8vo, Pp. 386.

4. *Eczema: its Nature and Treatment.* By TILBURY FOX, M.D., Lond., M.R.C.P. London: Henry Renshaw, 356, Strand. 1870. Pp. 68.
5. *Notes on the Treatment of Skin Diseases.* By ROBERT LIVEING, A.M. and M.D., Cantab. London: Longmans, Green, and Co., 1870. Pp. 90.

THE two monographs whose titles are first given are so essentially the same that we shall confine our remarks to one of them, and we will select Dr. Damon's as it has priority of publication, while, moreover, the care bestowed upon it by the publishers invites us to a closer acquaintance with its contents, which, we are glad to say, do not belie the exterior.

Dr. Damon does not now for the first time claim attention as a dermatologist, for he has already published *Photographs of Skin Diseases*, and this present work is only an instalment of a series of volumes on the other classes of skin diseases. Indeed quite lately the author has issued his second contribution devoted to the *Structural Lesions of the Skin*.

He makes but four primary divisions of skin diseases, viz.:—Class I. Neuroses of the Skin; Class II. Functional Diseases of the Cutaneous Glands; Class III. Inflammations of the Skin; Class IV. Structural Lesions of the Skin; and this present monograph is intended to supply all that is now positively known of the first of these classes of cutaneous affections, which are grouped as follows:—

	$\left\{ \begin{array}{l} \text{Dermalgia,} \\ \text{Prurigo,} \\ \text{Urticaria,} \\ \text{Zoster.} \end{array} \right.$
Hyperæsthesia	
Anæsthesia.	

There is little to call for special remark under the heads of dermalgia, *i.e.*, increased sensibility of the skin to touch, anæsthesia, or prurigo, except that with reference to the latter disease Dr. Damon does not follow Hebra, Squire, Fox, and others in insisting on the connexion of pediculi with prurigo senilis as cause and effect, and we certainly cannot endorse his opinion that “whenever these parasites do infest the skin, especially of old people, they multiply so rapidly that their existence becomes manifest upon the least inspection.” The discovery of the pediculi is often a matter of some difficulty, but of their being present as a

rule we have no doubt, and from some statistics of Mr. Hutchinson's out-patients, lately published by Mr. Nettleship, it appears that out of fifty-five cases of prurigo in adults and aged persons in which lice were looked for they were found in fifty-two. The evidence is very clear on this point, and warrants a stronger statement than that "sometimes pediculi infest old people and give rise to considerable pruritus." The truth, probably, lies between the two extremes, and a margin must, we think, be left for some cases which are a pure hyperæsthesia of the cutaneous nerves, and to which the term pruritus might distinctively be applied.

The most interesting of the cutaneous neuroses is undoubtedly herpes zoster, and Dr. Damon accordingly devotes to it a large share of attention, and collates the opinions of the best authorities, but, unfortunately, leaves us as much as ever in the dark as to its pathology.

Alibert had remarked that this cutaneous neuralgia in always being limited by the median line resembles the hemiplegias, and in connexion with this observation, and with Hebra's statement that in some cases the functions of the motor nerves are also interfered with, it is worth remembering that Dr. Duncan has reported in the *Journal of Cutaneous Medicine* for 1868 two cases in elderly women in which herpes zoster and hemiplegia coincided; both running the same course and ending in complete recovery within the month. In each case the herpes zoster occurred on the same side as the hemiplegia.

Various drugs are known which, when taken internally, will occasionally produce cutaneous eruptions, but we believe that Mr. Hutchinson was the first to point out the not infrequent coincidence, at least, of herpes zoster with the use of arsenic in the treatment of other skin affections. Mr. Hutchinson has now met with fifteen cases of such a coincidence, and besides records an instance of herpes preputialis and one of herpes labialis coming out during the administration of arsenic. Another case in point was related in the *Lancet* some time since, and not long ago a case came under our own observation. Arsenic had been prescribed for psoriasis gyrata, and after some weeks a well-marked patch of herpes zoster appeared above the left clavicle, and left behind it a crop of indolent papules, on the summits of which were seen the shrivelled remains of the vesicles. The herpetic cluster afterwards assumed the appearance of a patch of psoriasis, and resembled spots of eruption occurring elsewhere on the body. Various important facts on the subject of herpes frontalis

have been chronicled by different observers, and other points still remain to be cleared up, but on this question we must refer our readers to the work itself, and we hope that Dr. Damon's future essays will be of the same character as the present volume.

That Eczema, the "keystone of dermatology," is of paramount importance to understand, both as to its nature and cure, is implied by the recent appearance of three monographs devoted to this common disease and its congeners. Dr. McCall Anderson, Mr. Wilson, and Dr. Tilbury Fox, have each made it the groundwork on which to unfold the general pathology of skin diseases, and to illustrate their particular views and opinions. Of the four chapters into which Mr. Wilson's book is divided, the second chapter only is occupied with the natural history and treatment of Eczema itself. In the first chapter are discussed the general pathology, therapeutics, and classification of cutaneous affections, a re-cast of the text of his principal work; the third chapter takes up the "Ekzematous affections," Lichen, Impetigo, and Scabies, and the fourth chapter closes with essays, notes, and cases illustrative of eczema, including two papers, on eczema infantile, and on the nature and varieties of eczema, read before the British Medical Association in 1856 and 1864. These were chiefly published as lectures in the *Journal of Cutaneous Medicine*, and Mr. Wilson has done well to amalgamate them into a text-book. Yet we cannot avoid suggesting that the mass of really valuable information which is scattered through the work would be still more available if it were condensed within smaller limits, and even with the advantage of clear typography, most readers would, we think, welcome the curtailment of at least 100 pages at the expense of some of the "padding" which is so plentifully interspersed. We greatly doubt too if his recent and withal somewhat inconsistent endeavours to revive the strictly classical spelling of Greek words will meet with general favour. Whatever may be thought of the adoption of the Greek spelling of *proper names* in a classical work by a scholar of Mr. Grote's authority, the wisdom of Mr. Wilson's sudden enthusiasm and hyper-zealous eagerness to alter the spelling of familiar words, long sanctioned by daily use, is at least open to question. We do not see what is gained by the change to ekzema, leichên, bouknemia, kakochymia, alkohol, &c., nor do we feel sufficiently grateful for being reminded that in the case of psoriasis (with which he takes the liberty of connecting etymologically our simple Saxon word—

sore) "we must endeavour to pronounce the *p* as well as the *s*," and "that the *a* should be pronounced long, namely, psoriāsīs." If at one page he refers to our "clinicle circle," and at another is content to speak of a "clinical" classification, why not be consistent, and invite his professional brethren to visit his "klinikle kykle?" But, passing by these minor points, we gladly acknowledge the fulness and accuracy of the descriptions of disease, based on an extended experience, and the extreme value of the minute details given under the head of treatment.

In the Lettsomian Lectures for the session 1869-70, reprinted from the *Lancet*, and published now in book-form, we welcome Dr. Fox again as an author with pleasure. His writings have always seemed to us to be impressed with good sense and clearness of judgment, and while he takes full cognizance of the labours of others, he avoids the glaring faults into which many dermatologists fall. We allude more particularly to the tendency to introduce new names or to re-apply old terms in novel ways, and to the evil practice of regarding skin diseases in a piecemeal manner and not in their entirety, whereby mere *stages* in the course of a disease are raised to the position of distinct named varieties, and thus our conception of the true nature of the disease is rendered indistinct. Dr. Fox takes up his ground as an acknowledged follower and defender of Willan's views on eczema, and in vindicating the general correctness and accuracy of the doctrines of that distinguished man, he points out how grievously Willan's writings have been misinterpreted by most modern authors, not excepting Hebra and Wilson, the present representatives of Continental and English dermatology. It is evidently one of the main objects of the present work to illustrate the general principles on which cutaneous disease should be studied, eczema—the commonest disease of the skin—being selected as the accepted standard of reference, and we can congratulate Dr. Fox on the judicious manner in which he has carried out his purpose. Three lectures form the bulk of the book. In the first, the real significance of Willan's rendering of eczema, and the true nature and clinical varieties of eczema, are treated of. The second is occupied with the morbid anatomy and etiology of eczema, and its relation to certain constitutional conditions, and the treatment, both local and general, is carefully gone over in the concluding lecture.

Within a very small compass Dr. Liveing sketches concisely and

accurately an outline of all the principal skin diseases arranged in alphabetical order. A few pages on etiology, diagnosis, and treatment, precede the description; a short glossary follows, and a well-selected list of formulæ is appended, which are numbered for the sake of reference from the text. These "short notes for those who have little time to read" will, no doubt, as they deserve, find a ready acceptance among our hospital students; and the portability and neatness of the little volume are not its least recommendations.

1. *Ueber Sympathische Gesichtsstörungen.* Von. Dr. med. ALBERT MOOREN, dirigierendem Arzt der städtischen Augenklinik zu Düsseldorf. Berlin: August Hirschwald, 1869.
On Sympathetic Impairment of Vision, 8vo, pp. 169.
2. *Failure of Sight from Railway, and other Injuries of the Spine and Head; its Nature and Treatment, with a Physiological and Pathological Disquisition into the Influence of the Vaso-motor Nerves on the Circulation of the Blood in the Extreme Vessels.* By THOMAS WHARTON JONES, F.R.S., F.R.C.S.; Professor of Ophthalmic Medicine and Surgery in University College, London; Ophthalmic Surgeon to the Hospital, &c., &c. London: James Walton, 1869. Small 8vo, pp. 309.
3. *Archives of Ophthalmology and Otology.* Edited and published simultaneously in English and German. By Professor H. KNAPP, M.D., in New York, and Professor S. MOOS, M.D., in Heidelberg. Volume I., No. 1. New York: William Wood and Co. London: Trübner and Co.
4. *Transactions of the American Ophthalmological Society. Fourth and Fifth Annual Meetings. Do. Sixth Annual Meeting, containing also the Transactions of the American Otological Society* 1869.
5. *A Treatise on Intra-ocular Tumours. From original Clinical Observations and Anatomical Investigations.* (With one Chromo-lithographic and fifteen Lithographic Plates, containing very many Figures). By H. KNAPP, M.D.; late Professor of Ophthalmology, and Surgeon to the Ophthalmic Hospital in Heidelberg. Translated by S. COLE, M.D., of Chicago. New York: William Wood and Co. London: Trübner and Co. 8vo, pp. 323.
6. *A Treatise on the Diseases of the Eye.* By J. SOELBERG WELLS, Professor of Ophthalmology in King's College, London. 2nd Ed. London: John Churchill and Sons. 1870. 8vo, pp. 797.

OUR present batch of works on ocular subjects illustrates the existence of specialities within specialities, as well as the steadily-increasing interest displayed in ophthalmology. Dr. Mooren, of Düsseldorf, supplies us with a valuable monogram on sympathetic impairment of vision, which should be in the hands of every specialist. Dr. Knapp, of New York, gives us a well-translated work on the two forms of intra-ocular tumours, which alone have come under his notice, and which are, he believes, the only ones that do occur within the eye, viz., sarcoma of the choroid, which may be either unpigmented, or melanotic and glioma of the retina—a term made use of at present to denote principally that affection which has hitherto been called by us fungus hematodes. This work is a most welcome and valuable addition to medical literature in the English language, and is, we believe, the only monogram in our language on the subject. Professor Wharton Jones writes a voluminous and very learned essay explanatory of the failure of sight consequent upon injuries to the cerebro-spinal system. This essay might be considered as an independent appendix to Erichsen's work on *Railway and other Injuries of the Nervous System*, of which we gave an account in this Journal in November, 1867. The learned professor makes his essay the medium of reasserting and vindicating those experiments, observations, and doctrines respecting inflammation and the vaso-motor system which he put forward with so much force and ability upwards of twenty years previously. We regret that want of space forbids our entering into a discussion of the various topics treated of in this essay. We hope, however, that our unavoidable silence may induce our readers to peruse for themselves this highly interesting and instructive volume. We would direct particular attention to the author's remarkable discovery concerning the action of Calabar bean on the walls of veins.

With respect to the impairment of vision ensuing on injuries of the spine, Wharton Jones argues that it depends immediately upon disturbance of the circulation in the optic nervous apparatus, and that this disturbance is produced by the morbid changes in the spinal cord being transmitted to the muscular walls of the arteries by means of the sympathetic nerve. We should mention that this doctrine is not generally accepted, but that many hold that the morbid changes in the spinal cord are transmitted directly by continuity of structure to the brain and its efferent nerves.

We would take exception to the title on the back of this work—

Failure of Sight from Railway and other Accidents—inasmuch as the essay deals exclusively with accidents to the cerebro-spinal system. We would also desire to see some further confirmation of the remarkable ophthalmoscopic symptoms stated to have been observed; thus in some of the cases, while one half of the optic disc was white and atrophied, the other half was red and congested. Pigmentation is also recorded as of frequent occurrence after spinal injuries, which produce impaired vision. The optic disc in one instance is stated to have been of a “black leaden hue from granular pigmentous deposit.” The work is well brought out, and reflects credit on its publishers.

Our present list, as well as former ones, illustrates also the great interest in ophthalmic matters displayed in the United States of America. The American Ophthalmological Society sets an example which might be followed with advantage in our own country. Amongst the American publications on our list we have to welcome the first number of the *Archives of Ophthalmology and Otology*; and we do so most heartily, and trust the publication may have a long and prosperous career. If the succeeding numbers are at all like the present one, our hopes must be realized; if literary enterprise, combined with ability, deserve success, this periodical should succeed. The work is published in Carlsruhe and in New York simultaneously in German and in English. The number before us is profusely illustrated with three plates of chromolithographs and eight plates of plain lithographs; it contains twenty-seven original communications from various well-known authors in America and Europe; those communications are on the most varied subjects, and are characterized by erudition and practical experience. The last article in this number possesses great general interest; it relates the notes of what is stated to have been a metastatic abscess on the foot in consequence of primary otitis and otorrhea, which was induced, it is said, by the use of Weber's nasal douche.

The very high commendation which we bestowed upon the first edition of Professor Soelberg Wells' Treatise, is shown to have been well deserved; it has been translated into French and German, and now appears as a second edition, into which the distinguished author has incorporated the recent additions to our knowledge of ophthalmic surgery.

PART III.

QUARTERLY REPORTS.

REPORT ON SURGERY.*

By EDWARD H. BENNETT, M.D. and C.M., T.C.D., F.R.C.S.I., M.R.I.A. ; Surgeon to Sir P. Dun's Hospital ; University Anatomist and Lecturer on Clinical Surgery in the School of Physic.

THE communications made during the present year to the Royal Medical and Chirurgical Society of London, and to the Imperial Society of Surgery of Paris, contain much valuable information on the subject of vesical calculus. Of most importance is the analysis of 184 cases of stone in the bladder of the adult treated by lithotrity which Sir H. Thompson has submitted to the first-mentioned society. We are indebted to the *British Medical Journal* for our knowledge of this analysis, and of the discussion which followed its presentation to the Society, as the volume of the Medico-Chirurgical Transactions for the present session has not yet been published.

“The chief facts relative to the 184 cases were as follows :—They were consecutive in point of time, no case being omitted ; that all were adults, and mostly of advanced age ; that they included many individuals of very feeble health and constitution ; that they were chiefly British, although several were from other nations. The mean age of the 184 cases was no less than 61 years. The youngest was 22 years old. Only three were below 30 years. The oldest was 84 years. There were 46 cases of 70 years and upwards. With very few exceptions, all stones of an ounce and upward were reserved for lithotomy ; all obviously below that were crushed. Not one case was refused operation, not one was left unfinished, and in no instance was an operation of lithotrity completed by lithotomy.”^b

* The author of this Report, anxious that every contribution to surgical literature should be noticed, will be glad to receive any publications on surgical subjects. If sent to the correspondents of the Journal they will be forwarded.

^b British Medical Journal, June 4th, 1870.

The last statement must be modified, as we find that Sir H. Thompson admitted, in reply to a question from Mr. Hutchinson, that in one case he had removed two or three small calculi by lithotrity, and subsequently performed lithotomy, the patient having suffered from abscesses after the crushing operation. "The recoveries, reckoning every kind of casualty following the operation, were 93 per cent.; but omitting five cases of death, not by any means due to it, the mortality amounted to only 4 per cent. A second operation for recurrence of the stone was performed for 13 of the 184 cases. Of the stones removed, "122 were uric acid and the urates; 16 were mixed; 40 were phosphatic; 4 oxalate of lime; 1 cystic oxide; and 1 pure phosphate of lime."

Sir H. Thompson concludes from his experience that the rate of of mortality corresponds with augmentation in the size of the stone, and with the amount of existing disease and age on the part of the patient, the danger of fatal accident happening in the case of a fairly healthy patient suffering from a small stone being no greater than in any of the minor operations of surgery. Sir B. Brodie, on whose statistics Sir H. Thompson places most reliance, expresses the same opinion in his paper, published in 1855.* "My own experience has certainly led me to the conclusion that lithotrity, if prudently and carefully performed, with a due attention to minute circumstances, is liable to a smaller objection than almost any other of the capital operations of surgery." The opinion of Sir H. Thompson as to the cases suited for lithotrity agrees with that of Sir B. Brodie, while it is more accurately defined and expressed: "To apply lithotrity to all calculi obviously less than an ounce in weight, easily discoverable by sounding, and to operate on all larger ones by lithotomy." Sir B. Brodie expresses his opinion thus:—"The cases to which lithotrity is not applicable are very few indeed, and they are chiefly those in which, from the calculus having attained an unusual size, the danger and difficulty of lithotomy are so great, that no surgeon would willingly, nor otherwise than a matter of duty, undertake it." These opinions apply only to the case of stone in the adult.

M. Dolbeau^b advocates perineal lithotrity as the best mode of removing stone from the bladder, and submits the following facts as proof that the proceeding is entitled to be placed among the recognized operations in surgery. In 1863 he performed

* *Medico-Chirurgical Transactions*, 1855.

^b *Gazette des Hôpitaux*, No. 26, January, 1870.

his operation for the first time, and the patient rapidly recovered, and was alive and well last August. Since 1863 he has performed the operation on twenty-two patients with but one death. The fatal issue in the single case, the last of the series, may be fairly attributed to other causes than the consequences of the operation, for the patient died, on the fifteenth day after the operation, in an attack of insanity, during which he attempted to hang himself. This case afforded the opportunity of an examination of the parts involved in the operation, and of their exhibition to the Imperial Society of Surgery in Paris.

The steps of M. Dolbeau's operation are:—

1. An incision carried from the border of the anus forwards directly in the middle line, 2 centimetres ($\frac{3}{4}$ -inch) in extent, dividing the skin and subcutaneous tissue.

2. A puncture in the posterior angle of the wound, made to strike a staff in the urethra.

3. Gradual dilatation of the track of the puncture.

4. Dilatation of the neck of the bladder to the entire extent possible with the dilator, that is to say, less than 2 centimetres in diameter.

5. The crushing of the stone and extraction of numerous fragments.

The dilator used by M. Dolbeau is composed of six blades of steel, which, when closed, form a pointed tube, resembling a porte-crayon; these blades can be separated parallel to each other by a motion of the handle, until the tube measures, at the maximum dilatation, nine-tenths of an inch in diameter. The number of blades of the dilator has the effect of enabling it to exercise a circular dilatation perfectly regular in character, and this is strictly confined to the limits of dilatability of the neck of the bladder. The mode of reaching the bladder in this proceeding is the same as in Allerton's operation, except that a mechanical contrivance is substituted for the finger of the operator in dilating the neck of the bladder, a mode of proceeding which M. Dolbeau considers to be too sudden, and liable to the accident of false passage.

The specimen submitted to the Imperial Society by M. Dolbeau showed the parts taken from a patient 67 years of age, who had been operated on fifteen days before death. The bladder was found full of urine, its neck absolutely intact, and the mucus membrane healthy.

M. Dolbeau sums up his communication with the following

propositions, deduced from the examination of the pathological specimen:—

1. That it is possible to open a direct passage from the anus to the bladder without damage either to the bulb, the rectum, the neck of the bladder, or the vessels of the region.

2. That it is possible to introduce into the bladder instruments destined to crush a stone and to extract the fragments, without rupturing the neck of the bladder, or without destroying the power of retention of urine.

It is to be regretted that M. Dolbeau's communication does not contain the details of the 21 successful cases, especially as regards the size of the stones removed and the ages of the patients.

M. Chassaignac^a has also submitted to the Imperial Society the statistics of 13 cases of stone extracted by him with the *ecraseur* (*par l'ecrasment linéaire*). Of the 13 cases, 11 were adults, 1 an infant, and 1 a female. A single death occurred after the operation, which appeared to be due to renal suppuration, not fairly attributable to the operation alone. M. Chassaignac performs his operation by passing a curved trochar, guided by the finger, through the anterior wall of the rectum, so as to enter the groove of a staff placed in the bladder; the groove being reached, the trochar is conducted forwards along it, and is finally thrust out through the perineum behind the bulb of the urethra; through this tube the chain of the *ecraseur* is passed, and the parts included by it are divided. M. Chassaignac quotes the objections advanced against his operation on the occasion of his first communication to the Imperial Society on this subject in 1863, and refutes them by reference to his cases. The objections then advanced were:—

1. That the wound made by the *ecraseur* would become a fistula difficult to cure.

2. The difficulty of performing the operation.

3. The dangers of wound of the bulb, of purulent infection, and of division of the anal sphincter.

In the thirteen cases, neither hemorrhage, purulent infection, urinary infiltration, nor pelvic abscess, occurred. Without daring to assert that the bulb had not in any case been wounded, M. Chassaignac dismisses this objection in the absence of any hemorrhage in the cases operated on. The objection on the score of difficulty in the performance of the operation he maintains

^a *Gazette des Hôpitaux*, No. 19, 15th February, 1870.

to apply equally to any operation for stone, least perhaps to this method.

M. Guyon^a proposes, as deserving of trial, a method of treating large granulating surfaces by transplanting epidermis. The treatment was suggested to him by the consideration of the fact that islets of epidermis existing in a large granulating wound greatly expedite the cicatrization of it, as they form centres from which the formation of new tissue spreads, as well as from the borders of the surface. Two opinions are held by pathologists as to the spontaneous origin of such islets observed occasionally in extensive wounds; that there must be pre-existing in the granulating surface some part of the rete Malpighi, or surface of the true skin; secondly, that this condition is not absolutely necessary. M. Guyon states, that having had his attention drawn to the subject by the rapid healing of a wound in which islets occurred, he determined to attempt to produce similar islets in a wound of the forearm in which the skin had sloughed to a great extent. When the surface had become covered with granulations, and its border showed that cicatrization was commencing, a month having passed after the receipt of the injury, he raised, with the point of a lancet, from the opposite limb, two minute flaps of epidermis, as far as possible avoiding a wound of the true skin. The detachment of the flaps left only a florid spot of skin exposed. He placed them on the centre of the wound, at a little distance from each other, with their deep surface applied to the granulations, and fixed them by two straps of plaster which formed part of the dressing. On the next day they retained their place, in spite of profuse suppuration, appearing a little swollen and whiter than when transplanted. On the third day, they being still in place, M. Guyon raised a larger piece of epidermis, about one-tenth of an inch square, and placed it on the wound, some distance from them. On the next day the three flaps were adherent to the surface, and could be rubbed without detaching them.

Two days afterwards the first pieces looked more pale and thinner, and appeared to be extending at their borders; next day they had united, and formed a pale white patch with epidermis evidently forming around it. The third flap was now firmly adherent, and was surrounded by a little pale band; during the following days this band, and that around the spot formed by the

^a *Gazette des Hôpitaux*, No. 4, January 11, 1870.

two first pieces, extended more and more. In a fortnight all three spots were united, and the islet so formed continued to spread rapidly.

A second experiment, made on a wound the border of which had not shown signs of cicatrization, failed, the grafts of epidermis falling off after being some days slightly adherent.

In a paper read before the Medical Society of Berlin, Langenbeck^a describes the details of the operation of sub-hyoidean pharyngotomy performed by him:—

The patient, a woman of fifty years of age, was small and of fragile build. She had been healthy in youth, and had one child and two miscarriages. For some years she suffered from profuse menorrhagia at the menstrual periods, which ceased five months previous to the date of the operation, upon the removal of a uterine polypus. In October, 1867, she had difficulty of swallowing, and felt as if something was sticking in her throat. She had pain in the neck, and became gradually hoarse. Swallowing became very difficult, until at last she could no longer swallow solid food. Her breathing was greatly impeded, but she was free from attacks of asphyxia. She had lost flesh, and had the appearance of suffering, and was restless and excitable. The anterior region of the neck was curved forward, and was hard and swollen. The contour of the hyoid bone and of the larynx could not be either recognized by the eye or felt. If the tongue be depressed, a grayish red tumour, the surface of which is unevenly lobed, can be seen in the pharynx. Digital examination shows the tumour to be about the size of a pigeon's egg, of firm, elastic consistence, and that it quite fills the pharynx.

The attempt to push the finger behind the tumour causes great respiratory distress, and its lower end cannot be reached. Examination with the laryngoscope affords no further knowledge, as the tumour prevents any view of the larynx. From the possibility of passing the finger behind and to the sides of the tumour, it is believed to spring from epiglottis. On August 11, 1869, the patient was placed under chloroform, and the operation commenced by Langenbeck. He first performed tracheotomy, opening the trachea through its upper ring. In this step some difficulty arose from the uncertain position of the parts, and from the hardness and infiltration of the tissues covering the larynx. A tube being

^a Berliner Klinischen Wochenschrift, 1870, No. 2. We are indebted to Mr. H. R. Swanzy for this paper.

placed in the trachea, the second step of the operation was commenced. A small transverse incision was made close beneath the lower edge of the hyoid bone, and the sterno and omo-hyoid muscles were divided. The middle thyro-hyoid ligament was found almost absorbed, and replaced by very soft gelatinous tissue. By passing a finger into the pharynx, an assistant pushed the tumour forwards against the thyro-hyoid space, and the mucous membrane of the pharynx was divided. The epiglottis was now seen to be perfectly normal, and was drawn forwards through the wound by means of a strabismus hook; the tumour was then seen, and was likewise drawn out by a forceps. The tumour had its origin from the left aryteno-epiglottic fold, and reached from thence with a broad base to the left side of the pharynx, being so closely attached to the left arytenoid cartilage that the cartilage was drawn through the wound with it. The tumour was carefully separated from its connexions, the operation being attended with considerable hemorrhage. In order to prevent the entrance of blood into the larynx, a soft sponge was pressed through the wound, so as to cover the rima glottidis, and this was changed for a dry one when it became saturated with blood. After the tumour was dissected out but one artery required ligature, the bleeding ceasing spontaneously. The wound was closed with sutures, and the canula was secured in the trachea.

The tumour, which is of the size of a large pigeon's egg, is round, and of somewhat irregular shape, and of firm, elastic consistency; it appears from numerous inequalities and the intervening depressions not unlike a highly hypertrophied tonsil. Microscopically it has the structure of a fibro-myxoma, several myxomatous deposits being embedded in the elastic fibrous substance making up the bulk of the tumour.

The reaction following the operation was moderate, the temperature never rising over 38.8 C. in the evening. On the first day, on drinking, the fluid passed into the trachea, and it was therefore necessary to feed the patient three times a day with the œsophagus tube. Upon closing the canula the voice is distinctly audible, but is rough, deep, and hoarse. On the fourth day the wound had healed by primary union, except at the centre; through the opening, however, saliva and pus constantly exuded. On drinking, the fluids passed no more into the larynx, the closure of the glottis was, consequently, once more possible; everything ran, however, still above the epiglottis, out through the wound, so that nourishment had still to be given through the œsophagus tube.

During the next eight days the patient was troubled greatly by a bronchial catarrh, especially at night. On the twelfth day the closure of the wound in the neck had advanced so far that only a very little of the fluids swallowed passed through it. On taking out the canula, the respiration through the larynx was quite free; for safety it was however still retained. On the fourteenth day, without any apparent cause, great swelling and pain of the tongue, attended with febrile symptoms, came on, connected, as it appeared, with œdema of the glottis—at least the voice became again hoarse, and at last reduced to a whisper. All these symptoms, however, disappeared almost spontaneously after a few days.

On the twenty-third day the canula was removed, and the nourishment with the œsophagus tube discontinued. The patient recovered now very quickly, and was discharged as quite healed on the 18th September, after being five weeks in treatment. Her condition at that time was as follows:—The wound is quite cicatrized, with the exception of a narrow fistula to the right of the middle line below the hyoid bone, which leads into the larynx. She swallows solid, as well as fluid nourishment, without difficulty, complaining of a sensation of tension merely in the external cicatrix, and in the skin of the neck. With the laryngoscope the position of the cicatrix on the aryteno-epiglottic fold can be but indistinctly seen, inasmuch as the epiglottis conceals the entrance to the larynx more than usual, and is, moreover, drawn over somewhat to the left.

The statistics of the operation are too small to admit of forming an opinion as to its dangers, but still Langenbeck believes it is destined to take an important place among the operations on the larynx. He thinks the following diseased conditions would be indications for its adoption:—

1. Foreign bodies in the pharynx, whose removal by the mouth is impossible, but whose seat in the pharyngo-laryngeal cavity is certain.
2. Tumours in this part of the pharynx, which are situated upon the mucous membrane, with a broad base, or in the wall of the pharynx between the mucous membrane and the muscular layer.
3. Growths springing from the epiglottis, the aryteno-epiglottic folds, and from the arytenoid cartilages.

Langenbeck thinks it may be of great service in making amputation of the epiglottis possible in many cases of cancerous disease of that organ, where, until now, the surgeon has been satisfied with preventing suffocation by means of tracheotomy.

The operation was first described by Malgaigne in his *Manual of Operative Surgery*, published in 1835; but Vidal claims the credit of its invention, and states that Malgaigne derived the idea of the operation from him.^a The operation was first performed by a surgeon of the French Navy for the removal of a fibrous tumour of the epiglottis. Langenbeck performed the operation for the first time in July, 1862, for the removal of a fibrous tumour of the size of an apple, dislocating the thyroid cartilage downwards, and to the left side. The tumour had been previously operated on by galvano-cautery, but only partially removed. The patient suffered from difficulty of breathing, amounting sometimes to asphyxia. The tumour was removed, but the patient died on the evening of the second day. In the course of the operation tracheotomy had to be performed to prevent suffocation from the abundant hemorrhage; hence Langenbeck advises that the performance of the operation should commence with the introduction of a tube into the trachea, as in his last case reported above. In 1863^b Follin performed the operation successfully on a boy for the removal of a polypus of the larynx without tracheotomy. Débroux, of Orleans, in November, 1863, performed the operation on a man aged fifty-two, for polypus of the larynx. Débroux performed tracheotomy after the operation was concluded to guard against œdema of the glottis.^c The patient died on the seventh day of bronchitis, caused, in the opinion of the operator, by the tracheotomy, which, he thinks, might have been dispensed with.

^a Gazette des Hôpitaux, No. 103, 1859.

^b Gazette des Hôpitaux, 1864, p. 167, 343.

^c Gazette des Hôpitaux, 1864, p. 46.

REPORT ON MATERIA MEDICA AND THERAPEUTICS,*

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MATERIA MEDICA AND GENERAL THERAPEUTICS.

1. *Explosive Prescriptions*.—(a). *Oxide of Silver*.—℞ Argenti ox. gr. iss. ext. nuc. vom., gr. $\frac{1}{6}$; morph. mur., gr. $\frac{1}{32}$. M. G. B. H., of Chertsey, states, that having prepared pills of the Mr. formula with confection of roses, or with extract of gentian, the pills in a short time exploded with evolution of considerable heat. Permanganate of potash will sometimes act similarly. (*Pharm. Journ.*, Aug., 1868.) An analogous case lately occurred to Dr. Jackson, of Nottingham, and excited some interest at the time. The following prescription was made up:—℞ Arg. ox., gr. xlviii; morph. mur., gr. i.; ext. gent., q. s. M. Ft. pil., xxiv. The lady who received the pills, which were silvered, put the pill-box into her bosom. In three quarters of an hour a severe explosion took place; her clothes were burnt, her right breast severely scorched, and smoke issued freely from beneath her dress. A troublesome burn on the breast remained for treatment.—(*Pharm. Journ.*, March, 1870.) It has long been known, that pills made with oxide of silver and creasote (or carbolic acid; Boettger) are liable to become very hot, or even to inflame, and a dispenser has been astonished by seeing the lid of a box which contained such pills suddenly blown off, and the pills sent rolling over the counter.

(β). *Chlorate of Potassium*.—The following prescription gave rise to a violent explosion on being made up by trituration in a rough wedgewood mortar;—℞ Pot. chlor., oz. iss.; ac. tannici, 3 iss.; olei gualtheriæ gtt., xx. M.—(*Pharm Journ.*, Oct. 1870.) Again, a mixture of chlorate of potassium and catechu, prescribed as a dentifrice, occasioned a violent explosion in the mortar in which it was rubbed up. Erhard's explosive powder for shells is composed of equal proportions of tannin and of chlorate of potassium.—(*Rev. de Thér. Med. Chir.*, No. 2, 1870.) Lastly, a "pharmacien" received the following prescription to dispense, viz.:—Pot. chlor., 8;

* The author of this Report, anxious that no contribution to the subjects of *Materia Medica and Therapeutics* should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the *Journal* (see page iv.) they will be forwarded.

hypophosph. of sodium, 4; syrup, 62; water, 125 parts. In order to expedite matters he vigorously triturated the salts in a mortar, and the result naturally was that he received some wounds on the body, while the pestle was thrown to a distance. The two salts should, of course, have been dissolved separately.—(*Journ. de Ph. et de Ch.* Nov. 1869.) These and similar reactions depend on the facility with which oxide of silver and chlorate of potassium part with their oxygen to organic matter, and the consequent elevation of temperature due to the rapid decomposition of the salt.

2. *Anæsthetics.*—(a). *Bichloride of Methylene.*—According to Mr. Bader, bichloride of methylene (CH_2Cl_2) seems, in eye surgery, to fulfil the objects of an anæsthetic most thoroughly, and Mr. Philip Miall believes that, in small operations, it is decidedly preferable to chloroform, and for larger ones quite as useful. As the result of the experience of 100 cases Mr. Miall finds that insensibility in adults was generally produced in from two to five minutes, and that one dose of a drachm, and even less with females, was generally sufficient to produce anæsthesia. Complete recovery not unfrequently was established within three minutes after the commencement of inhalation. As compared with chloroform, half the quantity of the bichloride was found to operate in about half the time. Vomiting occurred 42 times out of 97; nausea merely, 6 times. Up to this time but two deaths have been laid to its discredit—one, in a man aged 30, operated on for excision of the upper jaw; the other, a healthy-looking man of 40, who underwent an operation for double iridectomy.—(*Brit. Med. Journ.*, Jan. 1, July 2, 1870).

(β). *Chloroform.*—In an article on the administration of chloroform, (*Brit. Med. Journ.*, Dec. 4, 1869,) the signs of danger enumerated are, lividity of the face, stertorous respiration, irregular gasping respiration, feeble pulse, and death-like pallor—"the most dangerous sign of all." The remedial means specially enjoined are stimulation by flipping with the corner of a damp towel on the cheeks, chest, abdomen, and limbs, artificial respiration, if possible, by a catheter introduced into the trachea, and brandy enemata.

Deaths from chloroform, even in the most skilful hands, are still sufficiently frequent to demand every care in its administration, and in the *Brit. Med. Journ.*, for July, 2, 1870, a table is furnished of deaths from chloroform in the United Kingdom during the last eighteen months. Out of a total of 17 deaths, 8 occurred from anæsthesia produced for somewhat slight, though often painful, operations.

Though there can be no doubt that, notwithstanding the most perfect management, the administration of chloroform is not without its risks, it may well be asked "are all such cases of syncope which take place during operations, and which end, or do not end, in death, the result of the action of the chloroform which happens to be used at the time?" One of the last papers from the pen of the late Sir J. Simpson contains remarks on a case of sudden death in ovariectomy while the patient was under the influence of chloroform.—(*Brit. Med. Journ.*, Feb. 26, 1870.*) It is there conclusively shown that "before the introduction of anæsthetics, patients sometimes died from syncope upon the operating table, both immediately before and after the operation was commenced, and under conditions and circumstances which in modern times, when anæsthetics are almost universally employed in operations, would be not unnaturally described and regarded as deaths from chloroform."

And the cases quoted "teach us, at least, that caution is required in our reasoning and inferences, seeing that death may occur, and has occurred, in operations without chloroform, and with phenomena quite similar to those ascribed to the action of chloroform." With this view before us it cannot be expected that any anæsthetic will ever be discovered which will be deemed absolutely guiltless of harm, for it will scarcely be possible to distinguish between the inevitable accidents of surgery and the casualties due to the particular anæsthetic employed.

(γ). *Methylic Ether*.— $\left. \begin{array}{l} \text{C H}_3 \\ \text{C H}_3 \end{array} \right\} \text{O}$. Dr. Richardson proposes the use of ethylic (*i.e.*, ordinary) ether saturated with methylic ether gas,^b as an anæsthetic. About two drachms are inhaled from domette in a simple mouth-piece. Two peculiarities of its action are—(1.) It produces quick relaxation of the muscles. (2.) While the patients under its influence are unconscious of pain, they are capable of performing what appear to be conscious acts, but which are, on recovery, entirely forgotten. The sleep is induced within one and a half minutes, and recovery is perfected as quickly.

* The last literary effort of Sir J. Simpson—composed almost on his death-bed—was a vigorous and impartial vindication of his own position in reference to the history of anæsthetics, written in answer to the unfounded imputations cast upon him by Dr. Jacob Bigelow.

^b This solution is sometimes incorrectly termed methyl-ethylic ether. The ether is charged with the gas at a temperature of 32° F., and the mixture is at once bottled and firmly corked down.

There is no appearance of asphyxia and the pulse undergoes little alteration. It has been employed in dentistry, and is probably well suited for short operations.—(*Lancet*, March 19, 1870.)

(δ). *Nitrous Oxide*.— N_2O . This gas can now be had, liquefied, in jars weighing 9 lbs., 12 by $3\frac{1}{2}$ inches, and containing 100 gallons of gas. Mr. Coleman has kept a patient an hour unconscious under its influence. About 500,000 patients in all have been subjected to its agency.—(*Edinb. Med. Journ.*, May, 1870.) Mr. C. J. Fox advocates the use of nitrous oxide in surgery for these reasons—(1.) Its safety. (2.) The rapidity with which anæsthesia can be induced, viz., from 50 to 100 seconds. (3.) The readiness with which a patient can either be kept in the anæsthetic state for a prolonged period, or, if wished, can be promptly and thoroughly awakened. (4.) Because it is pleasant to inhale. (5.) Because recovery is usually bright, pleasant, and complete. (6.) Because sickness has never, to his knowledge, occurred during the administration of the anæsthetic, and but rarely afterwards.—(*Lancet*, April 2, 1870.)

3. *Apomorphia*.— $C_{17}H_{17}NO_2 = \text{morphia } C_{17}H_{19}NO_3$ minus a molecule of water. Last year Drs. Matthiessen and Wright announced the discovery of this new base, which was obtained by the prolonged action of strong hydrochloric acid on morphia at a high temperature. Both chemically and physiologically it differs in nearly all respects from morphia, and was found to possess a remarkably powerful emetic action, but not dangerously depressant. From Dr. Gee's experiments, $\frac{1}{10}$ gr. subcutaneously, was considered the average dose for an adult, but Dr. F. M. Pierce finds that the desired effect is readily obtained with $\frac{1}{15}$ gr., or less. About $\frac{1}{6}$ or $\frac{1}{4}$ gr. may be given by the mouth. Vomiting occurs in from five to twelve minutes, and the vomiting is not followed by nausea. It causes dilatation of the pupil. Dr. Pierce uses a solution of 1 gr. of the hydrochlorate dissolved in 200 minims of water, and, in the case of children, injects 5 m. of this solution = $\frac{1}{10}$ gr. As an emetic, apomorphia stands alone in the smallness of the dose required; in the certainty, rapidity, and completeness of its action; and in its non-irritating qualities. At present, unfortunately, it is very costly.—(*Brit. Med. Journ.*, June 19, 1869; Feb. 26, 1870.)

4. *Morphia, Subcutaneous Injection of*.—Mr. Martindale recommends an aqueous neutral solution of the *recently prepared* acetate, 1 gr. in 6 minims. The bimeconate and citrate of morphia are also very soluble. The hydrochlorate and sulphate each require

upwards of 16 parts of water to hold them in solution.—(*Pharm. Journ.*, Feb., 1870.) Dr. E. T. Wilson lays special stress on three points—(1.) That the solvent for the morphia should be distilled water *only*. (2.) That the initial dose should be much smaller than that usually given. (3.) That the injection should be performed slowly. The muriate is preferable to the acetate for strong solutions, for it readily dissolves with heat, and remains so; 6 gr. of the muriate to 3 i. 3iss. appears to be the practical limit in distilled water. As a rare accident, he has twice met with a sudden and localized attack of urticaria.—(*St. George's Hosp. Rep.*, 1869.)

5. *Bromide of Potassium*. (See also Art. 18.)—*Adulterations*.—(See last Report, *Dubl. Quart. Journ.*, Feb., 1870, Art. 17.) Mr. A. E. Davies tested 10 samples obtained from Manchester, Warrington, London, and Edinburgh. The impurities, consisting of chloride and iodide of potassium, bromate of potassium, sulphate of potassium, and water, ranged from 13·75 per cent. to 31 per cent. Neither caustic potash nor carbonate of potassium were detected in any of the specimens. Chloride and sulphate of potassium were found in all, the former generally in considerable quantity, viz., 7·46 per cent. to 24·93 per cent.; bromate of potassium in all except one; iodide of potassium in 4 out of the 10 samples.—(*Lancet*, May 21, 1870.) The editor of the *Practitioner* lately instituted an inquiry into the average purity of this drug, as sold in London. Of the 13 specimens examined, 11 contained bromide, chloride, and iodide of potassium, and all included sulphates of potassium and sodium; 4 exhibited minute traces of bromate, and 7 of carbonate of potassium. No other impurities were found.—(*Practitioner*, May, 1870.)

6. *Carbolic Acid*.—Professor Bardeleben found that when externally applied in surgical cases carbolic acid was absorbed and acted poisonously in about 1 case out of 10. This poisonous action was revealed often so early as the second day by a peculiar effect on the urine which, pale at first, became gradually darker on standing. No albumen was present in the urine, but the patients lost appetite and strength. He recommends as a substitute the sulphocarbolate of zinc, first employed by Wood. Mr. Lister states that he has never observed the peculiar dark urine since the paste was replaced by the lac plaster.—(*Edinb. Med. Journ.*, May, 1870.)

7. *Chlorodyne*.—Quite an extended controversy has been going on for some time in the pages of the *Pharmaceutical Journal* as to the composition of this popular medicine. The most decisive communication is one from Mr. E. Smith, F.C.S., of Torquay, which appeared in the January number. Mr. Smith made a careful qualitative and quantitative examination of Dr. Collis Browne's chlorodyne, and puts its composition as follows:—℞ Chlorof. 3 iv.; morph. mur., gr. xx.; aeth. rect., 3 ii.; ol. menth. pip. m., viii.; ac. hydrocy. dil., 3 iv.; tinct. capsici., 3 vi.; mucil. acac., 3 i.; theriacæ ad 3 iv. M. This formula does not give so dark a compound as the original, because the latter contains caramel. Tinct. Cann. Ind. does not appear to be present, as the alcoholic extract is soluble in water. No evidence of atropia was obtained. Hence Dr. Ogden's and Mr. Squire's formulæ do not correspond with the real nature of chlorodyne.

"A Provincial" remarks in the February number that the case of poisoning by Dr. Collis Browne's chlorodyne, detailed and commented on by Dr. Dowse, proves most distinctly and indisputably that chlorodyne in *sufficient doses* does *contract the pupil*, notwithstanding Dr. Browne's positive counter-assertion. Dr. Dowse adheres to his belief that chlorodyne contains belladonna or atropia, and points out that the negative evidence of Mr. Smith's evidence is not, of course, conclusive. He grounds his opinion on physiological reasons and on repeated clinical experience on the action of chlorodyne with and without belladonna. "Another Provincial" agrees, in the main, with Dr. Dowse's views, as also in supposing it probable that tobacco enters into the composition of this favourite anodyne.—(*Pharm. Journ.*, April, and June 15, 1870.)

8. *Glycerine*.—Dr. Hager is convinced that glycerine often determines, particularly in regions where the skin is thin and delicate, erythematous and other eruptions. Hager found in a glycerine which produced irritation oxalic and formic acids, and in some specimens ammonia. Besides these impurities, M. Schepky has verified, in glycerine reputed to be pure, the presence of nitric acid, fatty volatile acids, and alkalies; traces of chlorine,^a lime, and sulphuric acid sometimes exist in glycerine which has not been distilled.—(*Rev. de Thér. Med. Chir.*, No. 2, 1870.) M. Pérutz states that butyric acid can easily be detected by gently heating the glycerine with a little alcohol and strong sulphuric acid. If

^a Chlorine is sometimes used for the purpose of bleaching glycerine.

butyric acid be present the pine-apple odour of butyric ether will be developed. According to Mr. J. Watts, the foreign or Vienna glycerine is apt to contain chloride of calcium, as much as one gr. in $\frac{3}{4}$ iii. An old specimen of yellow glycerine in my possession, labelled "pure glycerine" gave a distinct precipitate with nitrate of silver, soluble in ammonia, and a slight cloudiness with oxalate of ammonium.

9. *Liquor Hydrargyri Perchloridi*.—Mr. Martindale concludes from his experiments that this "liquor" does not contain any corrosive sublimate as such, but a double salt in solution, viz.; sal-alembroth, $2\text{NH}_4\text{Cl}$, HgCl_2 , H_2O . Among the tests mentioned the most important is, that the B. P. liquor does not combine with albumen until some time has elapsed, thus differing from corrosive sublimate alone. A simple solution of corrosive sublimate in distilled water is perfectly stable; if spirit be added, which is quite unnecessary, calomel is formed and precipitated after a time.—(*Pharm. Journ.*, March, 1870.)

10. *Mustard*.—According to M. Commaille, as in *black* mustard sulphocyanate of allyl is produced by the action of myrosine on myronate of potassium, so in *white* mustard seeds there are two principles, sinapisine and myrosine. Sinapisine is a crystallizable, sulphurated body, soluble in alcohol, and by decomposition with myrosine, it yields sulphocyanate of sinapine, a base not yet obtained in the dry state. Sinapine is readily decomposed into sinapic acid, and a new base, sinkaline.—(*Journ. de Pharm. et de Chimie.*, Janv., 1870.)

11. *Patent Medicines*.—(a). *Steedman's Soothing Powders*.—The composition of these is calomel, 27·20; ash, 0·06; and organic matter, 72·74. The organic matter seemed to contain a trace of opium. 28 out of 72·74 parts of organic matter were soluble in water.—(*Med. Times and Gazette*, Jan. 1, 1870.)

(B). *Mrs. Winslow's Soothing Syrup*.—No morphia could be detected in it, and it left no fixed residue. A bottle containing 50 c. c. furnished—cane sugar, 26·8 grm.; absolute alcohol, 2·2 grm.; oil of aniseed, traces; water, 30 grm. A teaspoonful contains about 3 grains of absolute alcohol.—(*Med. Times and Gazette*, March 19, 1870).

12. *Toxic Action of Quinia*.—In the last Report four cases were quoted in which an erythematous rash followed the administration of small doses of quinia. Dr. T. Skinner records another

corroborative case:—A lady after taking $1\frac{1}{2}$ grains of sulphate of quinia was seized with a rigor, and a universal scarlatinoid eruption, attended with intolerable itching, came out. Exfoliation of the skin continued for nearly three months. Similar effects followed another dose given some months after. Curiously, in the same lady, $\frac{1}{24}$ gr. of strychnia subsequently caused analogous effects, except that the desquamation only lasted six weeks.—(*Lancet*, Feb. 5, 1870.) The five cases now on record have all occurred in women.

SPECIAL THERAPEUTICS.

13. *Atropia as an Antidote to Calabar Bean.*—Dr. T. Fraser shows, from experiments on rabbits and dogs, that, in them at least, the lethal action of physostigma may be prevented in a remarkable and perfect manner by the physiological action of atropia. The physostigma was administered *after, along with, and previous to*, a certain dose of atropia. Poisonous effects are speedily produced in rabbits by atropia in small quantities, if it be directly introduced into one of the superficial veins. In treating cases of poisoning by calabar bean in man, he recommends from $\frac{1}{50}$ to $\frac{1}{30}$ gr. of sulphate of atropia subcutaneously, to be repeated until the pupils are fully dilated, the pulse rate increased, &c.—(*Practitioner*, Febr., 1870.)

M. Costesé also concludes that eseria (the alkaloid of physostigma) is the positive antagonist of atropia. Its action is fugacious, and disappears in about two hours, or sooner, if the atropia have not exhausted its influence. A dose of $\frac{1}{3}$ of a milligramme (0.003 gr.) is sufficient to produce decided contraction of an artificial mydriasis.—(*Rep. de Pharm.*, Mars.)

14. *Atropia in Intestinal Obstruction.*—Dr. Philipson, in a very urgent case of obstruction from fæcal accumulation of ten days' standing, obtained complete relief to the symptoms, and full evacuations, by the internal administration of 5 m. liq. atrop. sulph. ($\frac{1}{24}$ gr.), with 60 grs. sulphate of magnesium, and 10 m. dilute sulphuric acid every three hours. Castor oil, enemata, &c., had previously failed to produce any good effect.—(*Lancet*, Febr., 1870.)

15. *Constipation, Belladonna in.*—In connexion with the preceding article, attention may be drawn to the value in which belladonna is held in the treatment of simple constipation by many physicians. Dr. A. Fleming, of Birmingham, noticed some years ago that, in

prescribing atropia, slight relaxation of the bowels frequently took place one to four days after the alkaloid was taken. If any constipation had existed it was removed, and sometimes even purging was produced. Dr. Fleming gave atropia ($\frac{1}{60}$ gr.) in pill or solution, and often in combination with some saline purgatives and tonics.—(*Brit. Med. Journ.*, Dec., 1865.) Dr. S. G. Armor also speaks most favourably of the services of belladonna in chronic constipation due to atony of the colon, and especially in cases of a nervous and hysterical constitution. In such cases $\frac{1}{50}$ to $\frac{1}{40}$ gr. of atropia, or $\frac{1}{4}$ gr. ext. bellad. two or three times daily will usually in a short time bring about easy and natural defecation.—(*Amer. Practitioner*, Jan. 1870). Dr. T. B. Nunneley adds his testimony in confirmation, and in dyspeptic constipation gives $\frac{1}{6}$ to $\frac{2}{3}$ gr. of the extract every morning on rising.—(*Practitioner*, April.)

Therapeutics of Chronic Constipation.—Dr. J. K. Spender, in a sensible paper on the treatment of constipation when it exists *per se* as a primary disorder of function, recommends the following judicious plan, which comprises four factors:—(a.) Minute and frequent doses of watery extract of aloes, not exceeding 1 gr.—very rarely, of extract of colocynth. (b.) A dose of sulphate of iron, gr. iss. to gr. ii., *always combined* with the aperient. (c.) Regulation of the diet. (d.) Constitutional exercise. He does not believe that any other drug can supply the place of the sulphate of iron, though belladonna may be occasionally useful as an auxiliary. At first he gives a pill, composed as above, three times a day, immediately after the principal meals, and gradually reduces the number and frequency of the pills as the intestinal functions are restored. The action is slow, requiring two, or perhaps three, days, but *nothing approaching to purgation* ought ever to be permitted. As a substitute in certain cases, Mist. ferri co., with dec. aloës co., in corresponding doses, may be adopted.—(*Med. Times and Gazette*, Febr. 19, 1870.)

16. *Blisters, in the Treatment of Neuralgia.*—(a.) *Facial Neuralgia.*—In the facial neuralgia of middle age, Dr. Anstie states that a blister placed over the branches of the cervico-occipital nerve at the nape of the neck will often succeed in relieving the pain and the secondary affections, when counter-irritation applied directly to the painful nerve, or at any indifferent point, is either valueless or positively injurious.—(*Lancet*, Febr. 1869.)

(β.) *Herpetic Neuralgia.*—Dr. J. K. Spender confirms Dr.

Anstie's view as to the seat where blisters should be applied in the treatment of neuralgia. For twelve years he has found no remedy, not even morphia hypodermically, so potent to give relief in the neuralgia of herpes zoster of the trunk, as a blister just on the side of the spine corresponding to the seat of the disease. The blister should be allowed to rise well, and be kept discharging for two or three days.—(*Practitioner*, April.)

17. *Blister, Severe Effects from.*—Dr. J. A. Campbell applied Burt's *liquor vesicatorius* on the chest of a middle-aged man suffering from persistent chronic bronchitis. In a few hours he was attacked with severe tenesmus and strangury, and was in a state of extreme prostration and mental depression. He complained of great pain at the root of the penis, and in the rectum, and said that the few drops of urine he passed felt as if they burned him. No priapism was caused. The tenesmus and strangury were soon relieved by appropriate treatment, but the soreness about the rectum and penis continued for two days. In other cases, fluid out of the same bottle had been repeatedly used without any bad effects following.—(*Brit. Med. Journ.*, June 4, 1870)

18. *Bromide of Potassium.*—(See, also, Art. 5.)—(a.) *Ague.*—Dr. Moxon thinks that this drug possesses a very remarkable power over ague, and he has had several instances of its successful use; two of these were in persons who had taken quinine for a length of time without benefit. It is not pretended that, for the general cure of ague, it would compete in advantages with quinine, but it is at least worth a trial, and it is interesting to compare this result with the power which iodine and iodide of potassium have shown in curing ague, as reported in Virchow's *Archiv.*, xlix. The suggestion of this application of bromide of potassium is due to a student, Mr. W. Buchanan.—(*Brit. Med. Journ.*, June 11, 1870.)

(β.) *Saturnine Intoxication.*—M. Rabuteau proposes the use of the bromide of potassium, or sodium, in saturnine affections, as nervous sedatives, and as eliminators of the metallic poison. M. Bucquoy has also given the bromide of potassium with most marked and rapid success in lead cases. Large doses were given at first, but, after a time, not more than 2 or 3 grammes.—(*Rep. de Pharm.*, 1869.)

(γ.) *Danger of Large Doses.*—M. Vulpian administered as much as 8 grm. of bromide of potassium to a woman affected with sciatic

and crural neuralgia. Doses of 4 and 6 grms. had produced no effect. Five days after taking the 8 grms. the patient experienced considerable weakness. She could not stand, nor even sit, except for a few moments. She complained of pain in the isthmus faucium, of difficulty of deglutition, and of loss of power over the rectum. A few days after the suppression of the bromide her strength returned, and the neuralgia disappeared. In another case of chronic chorea in a woman aged sixty-three, the dose was gradually increased up to 10 grms. a day. The same symptoms of alarming debility followed in this case; the patient regained her strength but imperfectly, and the chorea was not sensibly benefited. M. Vulpian has observed similar effects in two other cases—one, a woman aged fifty-three, affected with incomplete right hemiplegia, and the second, a chronic affection of the spinal cord. The efficacy of bromide of potassium was more marked in cerebral than in spinal diseases.—(*Rep. de Pharm.*, Avril, 1870.) Comp. last report, *Dub. Quart. Journ.*, Febr. 1870, p. 214.

19. *Carbolic Acid*.—(α.) *In Psoriasis*.—Dr. J. M'Nab reminds us that in a large number of cases this disease obstinately remains as a local affection, the constitutional causes on which it depended having been remedied, but having left behind them a depraved state of the skin sufficient to perpetuate the disease indefinitely, irrespective of all constitutional causes. He strongly recommends, in such cases, an ointment of one part by weight of acid to carbolic four parts of lard, melted and mixed. This ought to be applied every night at bed-time, with gutta-percha covering, and be continued until the scales disappear, &c. Zinc ointment may be finally used.—(*Lancet*, March 19, 1870.)

(β.) *Poisoning by*.—Dr. J. Wallace applied carbolic oil (1 to 8) to an abscess connected with morbus coxæ, in a child aged five. In about two months' time it was remarked that vomiting and dysphagia invariably followed each dressing, and on examining the urine he found it to possess a dark smoky tint, very similar to the appearance of the urine in bad scarlatinal nephritis. Nitric acid added to the boiling urine threw down a heavy, dark precipitate. No trace of albumen. This deposit of pigment invariably appeared after each dressing with the carbolic acid, and disappeared again in a few days. A fortnight after the above symptoms were noted he adopted Prof. Lister's most recent method of carbolic dressing by oilskin, coated with dextrine and shell lac, and carbolic

acid plaster; matters became more favourable and the urine resumed its normal appearance. (*Brit. Med. Journ.*, April 30th). Dr. Lightfoot has met with another case in which alarming symptoms resembling those of pyæmic poisoning clearly resulted from the application of a weak aqueous carbolic lotion (1 to 50). The symptoms were developed three successive times when the lotion was employed and gradually subsided on its removal. Vomiting was dangerously severe, so that the patient's life was almost despaired of, but the urine was not darkened in colour. Numerous observers have recently met with cases of poisoning in connexion with the use of carbolic acid, and it is very necessary to observe caution as to the too free external use of this agent. The *black* or darkened urine, which is the most constant symptom, has been shown to occur in an equally marked form, whether tar or some colourless preparation of it be the agent employed. The exact cause of the colouration is still an open question, but it is at least probable that the colouring matter is not derived from the blood. The constitutional disturbance is sometimes very grave, and seems to bear some connexion with different forms of solution of carbolic acid, the lac plaster appearing to be the safest, while a weak watery solution, freely used, apparently involves the most risk. (*Brit. Med. Journ.*, April 2nd and 30th, 1870.)

20. *Cod Liver Oil*.—Dr. Fonssagrives strongly recommends the following mode of administering the oil, viz.:—*Ol. morrh.* ℥iij., *Iodoform* gr. vii. essential oil of anise. gtt. x. Mix.

A correspondent in the *Pharm. Journ.* for April furnishes a formula for cod liver cream. Mix equal parts of the cod oil and mucilage of tragacanth ($\frac{1}{4}$ oz. elect. gum tragacanth to $\frac{3}{4}$ vi. water), sweeten, and add to each fluid ounce of the mixture, $\frac{3}{4}$ i. rectified spirit; essence of lemon, essence of almond, and oil of cassia, 1 drop each.

M. Pidoux gives some useful information as to the administration and action of this favourite medicine in phthisis. When the oil is well borne by the digestive organs, fattening is rapid, but the muscular force and the hæmatisation do not augment in proportion. Improvement in these directions takes place later, even after the suspension of the oil. The fat which is formed at first seems to be promoted to higher functions, and contributes to the development of muscular fibre and of nerve tissue. Cod-liver oil to act beneficially must be properly oxidized. Hence it succeeds much better in phthisical patients who can take exercise than in those who are

confined to their room or to bed. In the latter it is not well digested, appears in the stools, or perhaps causes the formation of aplastic fat. *i.e.*, fat incapable of organic promotion. Its ingestion should be accompanied by that of a stimulating and cordial liquid, and be accomplished as near as possible to the beginning of a meal. In order to obtain its full effects, its administration ought to be occasionally interrupted, and fifteen days a month will generally suffice. —(*From Bull. Gén. de Thér. in Ann. et Bull. de la Soc. de Méd. de Gand. Aout. 1869.*)

21. *Diabetes*.—Dr. Pavy, in an elaborate paper, seeks to establish that opium, morphia, and codeia, possess a direct controlling power over the elimination of sugar in diabetes. Codeia he prefers as being equally efficacious, while it does not exert the same narcotic or disturbing effects as opium or morphia. He begins with $\frac{1}{2}$ gr., but has given codeia to the extent of 10 grs. three times a day.—(*Guy's Hosp. Reports, 1870.*)

22. *Narcotics*.—A review of Dr. Fronmüller's clinical studies on the hypnotic action of narcotic medicines is published in the *Wochenb. der k. k. Gesellsch d. A. in Wien*. No. 6, 1870.

(a). *Opium*.—The conclusions of the author with respect to opium are, 1. Of the different sorts of opium, Smyrna opium is the most active; then follow the Egyptian, Persian, Constantinopolitan, German, Algerian, and lastly, Indian and French. 2. Of the alkaloids, morphia and its salts, together with the allied metamorphia, stand first as hypnotics; then come narcotine, papaverine, opianin, and codeia. Narcein and thebaia have little hypnotic action; the remainder are almost worthless. 3. Of the morphia preparations, after pure morphia rank the citrate and meconate. 4. Narcotine acts similarly to morphia, only it must be exhibited in from ten to twenty times greater doses. 5. Codeia resembles morphia in its hypnotic action, but must be given in from six to eight times as large a dose. 6. The remaining opium alkaloids, partly on account of their rare occurrence, partly on account of their costliness, and partly on account of their slight or altogether deficient power, are of no practical importance. 7. Opium, in substance, narcotises more on the following night—the alkaloids on the following morning. Vomiting was more decided after morphia than after opium; perspiration was better marked after the alkaloids; but opium proved more constipating. The urinary secretion was more abundant after opium than after the alkaloids. 8. Morphia

surpasses opium as an hypnotic; but narcosis was less decided after morphia than after opium. 9. For subcutaneous injection, acetate and meconate of morphia are to be preferred.

(β). *Cannabis indica*.—Indian hemp, of all the narcotics, is the one which produces a hypnosis most nearly resembling natural sleep, without restraining the secretions, or causing other unpleasant results. 2. Hemp is not so powerful nor so certain as opium. 3. It may be employed in all acute inflammatory affections and in typhus. 4. It is especially suited to substitute opium when the latter drug fails. 5. The best mode of administering it is the alcoholic extract in pills, with the addition of a little powdered cannabis indica. As a minimum hypnotic dose, 4 grs. (in four 1 gr. pills), are requisite. It is often necessary suddenly to increase the dose. 6. The effects of Indian hemp on the skin, kidneys, and sexual organs are of no practical significance.

(γ). *Hyoscyamus*.—Henbane ranks third among the hypnotics, and is particularly indicated for the purpose of allaying catarrhal irritation. It does not constipate, favours perspiration, diminishes the urinary secretion, and dilates the pupil. Of the remaining hypnotics, *Lactuca* holds the lowest place. *Lactucarium*, especially the English and German, is the best preparation; dose 10 to 30 grs. *Lupulin*, *nutmeg*, *solanin*, *coca* and *cocain*, and *bromide of potassium* were found to be destitute of hypnotic value.

23. *Opium, Physiological action of*.—Dr. Weir Mitchell has made a number of experiments, chiefly on birds, and compares his results with those of Bernard, Baxt, Harley, and others, and concludes that—(1.) Birds, viz., ducks, chickens, and pigeons are never poisoned by crude opium, extract of opium, or acetum opii, when given internally. Salts of morphia must be given in enormous doses to produce fatal effects. (2.) Morphia salts, in excessive amounts hypodermically, never cause sleep or stupor, but act as excitants on the motor centres. (3.) Thebaia is a tetanising agent, only inferior in energy to strychnia and brucia. Narcotine, almost inert in man, destroy birds, when employed hypodermically, in doses of 2 to 7 grs. Codeia is a fatal convulsing agent in pigeons. Narceia has little or no perceptible influence. None of these agents cause sleep in the pigeon, duck, or chicken. Opium and morphia are very slowly absorbed by birds. Dr. Mitchell mentions incidentally that atropia, when dropped into the eye, or given internally, has no visible influence on the pigeon's iris, except in some

profound poisonings, to cause a curious swift alternate contraction and dilatation within a very small range of movement.—(*Amer. Journ. of Med. Sci.*, Jan. 1870). Compare Report, August, 1869, Art. 11.

24. *Nitrite of Amyl*.— $C_5H_{11}NO_2$. This compound, which has recently been introduced into practice, is a pale straw-coloured volatile, inflammable liquid, with an odour like that of over-ripe pears. It is prepared by acting on fusel oil with nitric acid. It has been examined experimentally by Dr. B. W. Richardson, and found, when inhaled, to act immediately as a powerful stimulant to the heart, and as an excitant of vascular action is more energetic than any other known agent. A little of it applied to the nostrils causes an instantaneous and extraordinary flushing of the face, and amplification of the pulse. Within the last year Dr. Brunton and Dr. Anstie have employed this new stimulant in *angina pectoris* with the most immediate and gratifying success. From his physiological researches, Dr. Brunton concludes that nitrite of amyl is of great value as a means of affording instant relief in all cases in which pain is due to arterial spasm. A few drops may be inhaled at a time. In Dr. Anstie's case, during an anginal spasm, the sufferer took one long and powerful inspiration through one nostril from a half-ounce bottle of the drug. After a pause of a few seconds the characteristic flushing of the face and sense of fulness in the head were induced, and the patient instantly passed from agony into a state of perfect calm repose. This experiment was several times repeated on the recurrence of the heart-pang, and always with complete success. (*Brit. Med. Journ.*, Feb. 26, 1870.)

25. *Phosphorus—Turpentine as an Antidote*.—In the Reports for August, 1869, and February, 1870, some notice was taken of M. Personne's experiments on oil of turpentine as an antidote to phosphorus. MM. Curie and Vigier cannot agree with M. Personne, either as to his theory or his experiments. They experimented on rabbits, which do not vomit, and also on dogs, and they assert that when care is taken to tie the œsophagus of dogs, or to replace approximately the quantities rejected by vomiting, these animals speedily succumb in spite of the employment of oil of turpentine. They object also that the quantity of oxygen which a poisonous dose of phosphorus could possibly abstract from the blood is too small to be the cause of death, and they deny that the blood in the

arterial system is, in such cases, black. (*Journ. de Ph. et de Chimie*, Jan. 1870).

26. *Santoniac Acid (Santonine)*—*On Hallucinations from*.—Dr. Edmund Rose (*Archiv. Path. Anat.* Band xxviii., Heft 1 & 2), has made some curious investigations on the visual illusions resulting from santonine. He pointed out some years ago that the phenomena were reducible to three perfectly distinct classes, viz:—1. Incapability of recognizing violet light, so that the spectrum appears curtailed at the violet end, and in all mixtures containing violet and yellow the complementary yellow seems to predominate; hence the phrase, “yellow-sight.” The patient is in fact colour-blind to the violet rays. 2. Inability to distinguish between dissimilar colours, and as all objects appear of a dark blueish shade this stage may be termed that of “violet-sight.” The violet-sighted sees every colour, but he *confounds* the colours systematically. 3. Hallucinations^a *i.e.*, actual sensations without objective stimuli; a class of phenomena which are independent of the entrance of light, and are almost exclusively and always most clearly and beautifully observed in perfect darkness. The present paper is devoted to the last group of phenomena. Doses of from 6 grs. of santonate of soda to 20 grs. of pure santonine were taken by various patients, and the effects noted. Incidentally Dr. Rose points out that partial colour-blindness but not colour-confusion may be induced by either the internal or external use of atropia, and that by the external application of atropia to one eye the other becomes impaired in its apprehension of colour. Atropia cannot produce the phenomena of Daltonism, as a strong dose of santonine can. The function of the retina depends on its circulation, and the colour-blindness is the first symptom of disturbance in that function. The colour-blindness of santonine is not connected with the nerves of the iris (Zöllner). The hallucinations in santonine intoxication are completely independent of both “yellow-sight” and “violet-sight,” and were observed in scarcely one-third of the cases. But, though infrequent and irregular in appearance, they may occur coincidently with these other perturbations of vision. Less frequently, hallucinations of the senses of touch and even of taste and smell are met with. From former experiments it appeared that of 30 cases, there were reported—

^a An “hallucination” is the conviction of a sensation when the organ of sense is not excited objectively; an “illusion” is the reaction of the brain upon external impressions which are misapprehended by the organs of sense. Illusions are hindered while hallucinations are called forth by excluding the excitement of the senses.

Yellow-sight in all, 30; violet-sight in 19; nausea and vomiting in 14; dizzy feelings, lassitude and prostration, in 9; visions in 8; hallucinations of smell in 6; hallucinations of taste in 5; abnormal feelings and pains in the head in 8; lowering of the pulse in 2. (*Brit. Journ. of Homœopathy*, April, 1869.)

CHLORAL.

Since the last Report in the February number of this Journal, a multitude of communications on chloral have appeared in the various periodicals, but in a limited space it will not be possible to do more than chronicle a few of the most important points which have been raised.

Preparation.—Some months ago M. Roussin proposed certain modifications in Dumas' and Liebig's process with a view to the more rapid production of chloral, but M. Personne has contested M. Roussin's suggestions, and by adhering exactly to Dumas' process obtained more than double the quantity of chloral from the same weight of alcohol that M. Roussin succeeded in procuring.

Varieties of.—M. Personne further showed that M. Roussin's supposed hydrate of chloral is not pure, but includes a quantity of alcohol, and is, in fact, a definite combination of anhydrous chloral with alcohol—a species of acetal, $C_2H Cl_3 O$, $C_2 H_6 O$. A commission, composed of MM. Roucher, Lebaigue, and Jungfleisch, confirmed M. Personne's statements in every particular, and both synthesis and analysis tend to establish the correctness of the formula which he proposed for M. Roussin's compound. The alcoholate possesses higher melting and boiling points, a lower density is less soluble in water, and contains only 76·3 per cent. of chloral, whereas the hydrate contains 90 per cent. (*Journ. de Pharm. et de Chim.*, Fevr. Mars., 1870.) Dr. Hofmann finds that some of the German chloral has the composition, $C_4 H_3 Cl_3 O$, and is, indeed, the chlorine derivative of crotonic aldehyd ($C_4 H_6 O$.) This new body owes its origin to the presence of aldehyd in the spirit from which the chloral is made; aldehyd acted upon by the hydrochloric acid liberated in the process, yields crotonic aldehyd, $2 C_2 H_4 O - H_2 O = C_4 H_6 O$, and chlorine acting upon this latter substance produces the new chloral. (*Pharm. Journ.*, May 1870.) The knowledge of the existence of these distinct compounds is of practical moment, and may serve to explain in part the discrepancies which arise in the experience of different observers, since there will obviously be differences in the effects of these several bodies. It

has lately been announced that Liebreich has succeeded in obtaining perfectly pure chloral hydrate in transparent crystals, about the average size of fine Epsom salt crystals, and which can be had for a trifling increase in price.

Theory of its action.—The question whether chloral passes through the blood unchanged, or not, is still *sub judice*, and directly opposite opinions are maintained.

M. Personne supports Liebreich's view, and concludes that chloral, on its entry into the blood, is decomposed into formic acid and chloroform, which again is ultimately converted into chloride and formiate of sodium, the final products of its elimination. The odour of the blood conceals that of the chloroform, but he demonstrated its presence by using the process employed in toxicological research for chloroform. Neither chloral nor chloroform could be found in the urine, but the formiate of sodium eliminated in that secretion has the power of reducing the cupro-potassic solution. (*Journ. de Ph. et de Ch.*, Fevr., 1870). But, on the other side, Dr. A. Gamgee recently delivered an interesting lecture, in which he urges some very strong objections, chemical and physiological, against the probability of Liebreich's hypothesis of the action of chloral being due to the gradual development of chloroform. No doubt, chloral is readily decomposed by free caustic alkalies, but the blood does not contain any of these substances. The alkalinity of the blood is due chiefly to alkaline phosphate of sodium, and probably in part also to bicarbonate of sodium. Now with regard to the first salt, Dr. Gamgee finds, that even when it is heated to boiling point in contact with a solution of chloral, it fails to decompose it, while with regard to the second, it is only after the temperature has been raised above 70°C that chloroform is given off, the evolution becoming extremely free when the liquid is boiled. Moreover, the symptoms which are produced by small doses of chloral, are quite out of proportion with those which we can suppose would be caused by an equivalent quantity of chloroform existing in the system; and, in addition, when we contrast the action of chloroform and chloral, we find evidences of very great differences. For example, in the former instance reflex action is soon abolished; in the latter it appears often heightened, or nearly, if not quite, unimpaired.

Physiological effects.—Dr. J. R. Reynolds relates a case, in which very serious symptoms were produced by 45 to 50 grs. of chloral in a middle-aged lady who had previously taken several 10 and 15 gr. doses with benefit. The symptoms came on in an hour, and

were mainly those of extreme prostration, an intolerable sense of sinking, gasping, breathing, and confusion of thought, and weak, irregular, and intermittent pulse. Under suitable treatment the symptoms were relieved, but returned with increased severity in the course of an hour, while the mind wandered. Relief was again obtained by the administration of white of egg, stimulants, fresh air, &c. (*Practitioner*, March, 1870.)

Mr. Streatfield and Dr. Clifford Allbutt, call attention to the fact of the occasional postponement of the effects of chloral for twenty-four hours, as is also the case with morphia hypodermically, and Dr. Maund thinks that its occasional uncertainty of action may be explained by the knowledge, that its influence is resisted by those habituated to the use of alcohol. Mr. Waren Tay in one case observed, that when a stimulant (wine) was administered at the same time as the chloral, the effect of the latter was less marked.

Association with Chloroform.—M. Liégeois when administering chloroform in a minor operation, after chloral had been previously given, found that the association, so far from increasing the effects, gave rise to excitement which lasted as long as the inhalation continued. M. Giraldès administered a chloral draught to infants who had been chloroformed and remained very agitated, with the effect of producing peaceful sleep for from 5 to 11 hours. M. Girard-Teulon has observed the same excitement produced by giving chloral in children that have been etherized, as related by M. Liégeois, in those who have been chloroformed. (*Med. Times and Gazette*, April 23rd, 1870.)

Modes of Administration.—Mr. P. Squire states, and others add their testimony, that peppermint water sweetened with syrup of tolu covers the taste of chloral better than anything else. M. Limousin proposes to avoid the unpleasant taste and irritating qualities of chloral by administering it in gelatine capsules or in *dragées*, and a “prescriber” recommends the following form:—Hydrate of chloral 3 ss.; aq. chlorof. 3 ii. (sp. chlorof. ?); syr. aurant. (or tolu) 3 i.—3 ii.; tr. zingib. m. vi.—xii.; water to 3 iss. Sir J. Simpson found that sickness is obviated by taking the chloral with lemon juice.

Therapeutic Uses.—The practical applications of chloral will fall, as before, under the heads of relaxing muscular spasm, of assuaging pain, and as a nervous sedative and hypnotic.

1. *Muscular Spasm.*—Dr. Richardson anticipated good results from its use in tetanus especially, and it has since been employed in

a few cases. Mr. Ballantyne in a traumatic case gave 3 ss. doses of chloral every four or five hours, with apparently good success, for out of nine cases of traumatic tetanus with which he has met, the only one which recovered was that treated by chloral. Mr. Waren Tay also tried chloral in a severe case of idiopathic tetanus of eight days' standing. Though the issue of the case was fatal on the tenth day after admission, relaxation of the tetanic spasms and the production of calm sleep constantly followed the use of the chloral. The fall of temperature too, as soon as the patient slept, was definite, and remained so as long as she was not roused up. Dr. More Madden is well satisfied with the use of chloral in cases of difficult labour from rigidity of the os uteri and soft parts.

2. *Pain.*—Its anodyne virtues seem to be subordinate to its hypnotic powers, and to be less certain in their operation. Smaller repeated doses appear to act more beneficially in this case than a single large dose; ten grs. as often as required may be suitably prescribed. Dr. Swift Walker claims for chloral a marvellous effect in cardialgia with excessive secretion of gastric juice, and also in allaying the sympathetic palpitation of dyspepsia, and Mr. Morgan has employed it in acute suffering from burns, ulcerated nodes, &c. Dr. Ogle confirms Liebreich's expectations of the value of chloral in the treatment of inflammatory painful affections, such as acute rheumatism, gout, muscular rheumatism, &c.; and Mr. Weedon Cooke is more than satisfied with the excellent results obtained in painful cases of cancer. Sir J. Simpson, Dr. More Madden, and Dr. Brady attest its use in painful affections of the bladder, and in ovarian pain the happiest effects have followed its use, after morphia and atropia had been abandoned.

3. *Nervous Sedative and Hypnotic.*—It would be superfluous to adduce further evidence in support of the efficacy of chloral as a hypnotic, for, in this respect, the general testimony is unanimous, and as its advantages and peculiarities were detailed in the last report, repetition on these points is needless now. Insufficient doses will sometimes cause delirium, and after the patient has fallen asleep, care should be taken not to wake him up suddenly, as delirium is apt to ensue (Sir J. Simpson). In delirium, especially the alcoholic form, its value is undoubted, and Dr. G. W. Balfour is most favourably impressed with its utility in delirium tremens. He records in the *Edinb. Med. Journ.* for May six cases illustrating the rapid beneficial effects in this disease of doses of chloral varying from thirty to forty-five grs.

In the eclampsia both of uræmia and of the puerperal state Dr. von Seydewitz checked the convulsions speedily by chloral, after chloroform inhalations and other means had failed, and in puerperal mania and other nervous affections incidental to delivery, a most favourable opinion of its use is entertained by Drs. More Madden, Alexander, A. M. Adams, and others. In a case of acute mania, recurring for the third time, and attended with complete insomnia, twenty-five grs. of chloral were productive of wonderfully good effects in Dr. Crawford's hands; opium and morphia had been previously tried extensively with the result of making the patient worse.

Dr. Tuke has most carefully and searchingly tested the action of chloral in certain cases of insanity, and was invariably satisfied with it in chronic cases of insanity in which violent outbursts of excitement occur. He confirms the possession by it of the various advantages already claimed for it, and "believes it to be the most valuable means of procuring sleep which has yet been introduced into the Pharmacopœia of the asylum physician." Dr. Hughes Bennett found that in many cases of advanced phthisis the relief to the cough and restlessness at night, with the production of sound sleep, were most marked under the use of chloral, which is also admirably adapted to relieve the severity of the paroxysms of hooping cough.

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

MR. PORTER, President.

Heart in Typhoid Fever.—DR. STOKES exhibited a heart taken from the body of a man who had been lately admitted into the Meath Hospital, suffering from the form of fever at present epidemic in Dublin, a fever of a typhoid character, and remarkable for the slowness of the convalescence, and the frequency of the relapses. The only local symptom which the patient complained of in this case was pain in the head, which was persistent, and remained unchanged until after the third week of his illness; there was no heat of head, or increased action of the carotids.

About the twelfth day of his illness, a few rose-coloured spots appeared over the abdomen, and a certain amount of tenderness was experienced in the ilio-cecal region; but there was no diarrhea, and the local tenderness did not continue. The bowels acted imperfectly, and required the occasional use of enemata; there was no cardiac or pulmonary affection, but a pallor pervaded the whole body.

In the middle of the fourth week, however, symptoms of an alarming nature showed themselves, and there were very evident signs of failure of the action of the heart, the first sound being one day inaudible, returning the next day, and then again disappearing.

The day before the man died, a condition of the circulation occurred, such as Dr. Stokes had never before witnessed, nor was he aware of its having been noticed in any of the works on fever. The pulse at the wrist was 70, regular and equable; but the beats of the heart were 140, exactly double the amount of the pulsation felt at the wrist, so that it was only every second stroke of the heart that was able to propel the wave of the blood to the wrist. The man died of syncope, signs of which existed for a few days before his decease.

Upon *post mortem* examination, it was found that there was no organic disease of the heart; no typhoid softening. The only abnormal condition noticed was the presence of a large and very firm coagulum in the left ventricle; the right ventricle and auricle were of a pale colour, the left being red.

In the small intestine, about eight inches above the valve, there were several spots of ulceration, and a few in the large intestine, close to the valve; yet this man had not had diarrhea, nor any symptom of enteric

disease, except the rose-coloured spots and the local tenderness.—*Dec. 4, 1869.*

Ovarian Tumour.—DR. KIDD said, the preparation he had to show the Society was taken from a woman, seen by him for the first time in the early part of the past summer. She was a patient in the Baggot-street Hospital, under the care of Dr. Tufnell. She was between fifty and sixty years of age, of a spare habit, but had not an unhealthy aspect, nor did she present the appearance of being exhausted or worn down by disease. She had a large abdominal tumour, which stood out prominently when she was lying upon her back, and the abdominal wall was so thin and so closely applied to it that its outline was very easily perceived. In addition, he noticed what appeared to be a second tumour lying in the left hypochondriac region, of about the size of the closed hand. The larger tumour could be moved through the abdomen with very great ease. He passed a sound into the uterus and found it to be normal as to size and position. It moved freely and independently of the tumour, and the movement of the tumour produced no effect upon the uterus. He also found that the tumour in the left hypochondriac region moved very freely. He could, with very slight management, carry it from the left to the right hypochondriac region. It appeared to pass freely across the upper portion of the larger tumour, and could be made to lie as completely in the right as it did in the left hypochondriac region. On drawing the attention of one of the students to this fact, the woman said she knew that the small tumour was movable; because when she turned in her bed, and lay on her right side, this tumour came to lie completely on the right side. It appeared to move quite independently of the larger tumour, in which he could not discern any movement while he carried the smaller one to the right hypochondrium. The opinion he then expressed, and which he had since heard, was one entertained by all who had seen the case, was, that this small tumour was a separate and independent one, having no connexion with the larger. Some of those who saw her believed it to be a floating kidney. His own opinion was, that it was a tumour of the great omentum. The woman remained in the Hospital for some days, and then left it; and some months afterwards she presented herself at the Coombe Hospital. She was so much altered in appearance that he did not recognize her. She stated she had been in another hospital, and had had a pessary introduced, and that ever since she suffered great pain; had a sense of bearing down, and pain in the left leg extending down to the heel. The limbs were swelled, and she had almost completely lost the power of her left side. She had all the appearance of a woman about to die, and in consequence of her extremely weakened condition, she was admitted into the hospital; and again, several who saw her believed the tumour in the left hypochondriac region was a separate one. She died after being some days in the hospital.

Post mortem examination.—The moment the abdominal walls were opened, the tumour, now on the table, fell out from its own weight. He at once put his hand into the left hypochondrium to seek for the tumour he had found so movable. To his surprise there was no tumour, and it turned out, that that which they believed to be a separate growth was really a projection or prominence on the surface of the large tumour which they now saw. On further examination he found the pedicle of the tumour was twisted—one complete turn—and the tumour presented, a few hours after death, the same black appearance it did at present. He was now in a position to correct the error that had been made in the diagnosis. The tumour was carried round by twisting it. They could turn it round completely, and in doing so, they observed that as it turned round on its axis the prominence on the upper and left side of the tumour was carried from the left side backwards, and to the right side, while the smoothness of the general body of the tumour allowed of its moving so freely as not to convey any sensation of motion to the hand placed on the abdominal wall. It was this movement of what appeared to be the small tumour, without any evident motion of the other that had led all who had seen the case during life into error as to its true nature. He believed the cause of death was that the tumour at some period had got completely twisted; there was a complete turn upon the pedicle; this had strangulated the tumour, and thus gave rise to the black appearance; and the woman died from irritative fever, produced by strangulation and sphacelation of the morbid growth.—*December 4, 1869.*

Multiple Ovarian Tumour—Rupture of one of its Cysts.—DR. KIDD said, they had a second patient, suffering from ovarian disease, in the Coombe Hospital at the same time as the woman from whose body the foregoing specimen was taken, and as she had died from peritonitis after the first tapping, and he had obtained the specimen, with the permission of the Society, he would now lay it before them.

It was an example of what Paget, and, following him, Dr. Arthur Farre, termed the multiple ovarian tumour, as distinguished from the multilocular. This woman had been in several hospitals in Dublin, and there was a considerable difference of opinion regarding her case. When she came to the Coombe Hospital she seemed to have ascites, but a tumour could be felt lying deep in the cavity of the abdomen, which some believed to be ovarian and others a fibrous tumour of the uterus. This tumour had a long pedicle; it floated freely in the cavity of the abdomen, and what gave rise to the idea of its being a fibrous tumour was this:—When the sound was placed in the uterus, and a sudden blow given to the tumour to move it, the shank of the sound turned nearly half round. Upon tapping they found that the fluid removed was such as was usually met with in ovarian disease—a thick, mucilaginous fluid,

and not like the fluid of ascites. She died from peritonitis, after the first tapping—an operation which some thought so simple and so safe.

On making a *post mortem* examination, they found that the tumour consisted of a series of cysts differing from the multilocular tumour, in not being inclosed in a parent cyst. One of these cysts had probably given way, and no doubt the fluid found in the abdomen was the produce of that cyst which had ruptured, and not only discharged its contents into the peritoneum, but had continued to secrete fluid till it filled the whole cavity.—*December, 4, 1869.*

Cyclopic Monster.—DR. JOHNSON exhibited the body of a fetus at the 8th month. It was small, but fairly formed. The head was small— $8\frac{1}{2}$ inches in circumference. The mouth presented a small circular opening. There was no nose in the proper situation; but at the point where the nasal bones are situated, a single orbit of large size, containing a body which evidently consisted of two eyes fused (as it were) into one. The circumference of the orbit was of a diamond shape, as though the inner side of each pair of lids had opened out and joined those of the opposite side above and below. The two pupils were represented by a dark spot; the fusion was not perfect, a slight constriction marking the point of junction.

Above this, and hanging down over it, was a pendulous proboscis, thicker at its free end, and tapering to its attachment to the frontal bone. It was perforated in the centre, and a probe could be passed down to its attachment to the frontal bone. The ears were perfect. The frontal bone was perfectly ossified—no suture. The point of bone to which the nasal bones are usually articulated, was present. On opening the head, by removing the calvarium, and raising the anterior lobes of the cerebrum, no olfactory nerves could be found. The dura matter was dissected off, and the following condition of things presented itself. A large nerve was traced through the sella tursica and frontal bone to the fused eyes. The fourth pair of nerves were present, but so fine, that it was impossible to preserve them. The third nerves took their usual course, entering the common orbit, one on each side. The sixth nerves were somewhat displaced, running into foramina toward the apex of the petrous portion of each temporal bone, instead of along the sides of the sphenoid in the cavernous grooves. The fifth and seventh were plainly seen and normal in situation.

There was one nerve arising from the base of the brain running directly forward, and passing on each side over the petrous portions of the temporal bones, and then piercing the floor of the fossa. It was most likely to be the second division of the fifth.

There was no nerve traceable to the proboscis, which was attached to the lower part of the frontal bone.

There were a few small prominences in the frontal bone, immediately

above this attachment, but they did not pierce the bone, but were openings into ill developed frontal sinuses.

The mother was a prostitute, and this was her second pregnancy; her first child was alive and well formed.—*December, 11, 1869.*

Fibrous Tumour.—DR. BARTON exhibited portions of a tumour which, he said, possessed some points of pathological, as well as of practical interest. They were taken from an infant brought to him in September last, by its mother. The infant was then two months old, and had a tumour about the size of a walnut at the back of its neck, which presented the appearance of a small spina bifida. He could make out, however, that it was not attached to the spine. The child was puny, and although apparently healthy, it was thin.

He directed the mother to take the child home, nourish it well, and bring it back after the lapse of a month. She waited two months, and then came back to him early in November, when he found that the tumour had grown to five times the size it was when he first saw it. It was plainly not connected with the spinal column, and was adherent to the integument. It was nearly as large as the child's head. The tumour was punctured with a curved needle; a little blood alone escaped. It did not appear to be malignant, and, as it was rapidly growing, it was thought proper to attempt its removal by the knife. Accordingly, on the 20th of November, an incision was made across the tumour, and it was partially dissected from the skin that covered it. The extreme feebleness of the infant prevented the completion of the operation at that time, and he therefore removed a portion of the tumour with a wire *écraseur* and covered the surface with carbolic acid. Finally, on last Monday, at a second operation, the whole of the tumour was removed, and in doing so a portion of the skin had to be sacrificed. It appeared to be a non-malignant tumour. It was very dense, presenting a true fibrous character, with a few oil globules, and a very few cells appearing through it. No suppuration had taken place. The child still survived and seemed likely to recover. The mother stated that about three weeks after the child was born she left it in charge of a little brother, who allowed it to fall, and she, coming in on the moment, found that the child had fallen on the floor and received a hurt on the back of its neck. Three days afterwards she noticed the tumour, which, up to that date she had not observed, and it grew rapidly from that time.—*December 11, 1869.*

Pancreatic Abscess.—DR. WALTER SMITH observed, that the specimen which he now presented to the Society had been taken from the body of a male subject, in the dissecting room of Trinity College. As no clinical history could be given of the case, any interest it possessed was necessarily connected with the *post mortem* appearances. While engaged in dissecting the viscera, some difficulty was found in exposing the pancreas,

owing to its close connexion with the neighbouring organs. A small abscess was found in the substance of the organ, near its splenic extremity; it contained a quantity of thick, greenish-yellow purulent matter. Near its duodenal end there were several small lymphatic glands, which also contained pus of the same nature. In the muscular portion of the diaphragm, above the spleen, there was also an abscess of considerable size; it did not penetrate the diaphragm. There was no appearance of inflammation in the adjoining pleural cavity. None of these abscesses appeared to spring directly from the pancreas. There was no morbid deposit in the stomach, liver, or kidneys, nor were the mesenteric glands enlarged.—*December 18, 1869.*

Periostitis, &c.—PROFESSOR M'DOWEL exhibited a specimen of that combination of inflammatory actions which his father had described in 1834, in the *Dublin Medical Journal*, under the title of "Periostitis with Synovitis." The case exemplified the great rapidity with which this terrible affection frequently proceeded to a fatal result, its duration being in this instance exactly twenty days. Its starting point was exposure to cold.

The subject of it, a young boy, having been for several days confined to the house, in the latter end of November, obtained permission to go out. He went to the Phoenix Park, and sat in the cold air for a very considerable time. When he returned home, he felt very much chilled; indeed, he said, that for several hours afterwards, he could not get rid of a general sensation of cold throughout his body. He felt ill for two or three days, with pains flying about him, and at last was suddenly attacked with intense pain in the right hip-joint. He was then admitted into the Whitworth Hospital, in a state of extreme agony, so that the most gentle motion of the joint could not be endured. There was no other articulation affected.

The attack resembled one of rheumatic fever; his pulse was very rapid, his skin very hot, and he had copious perspiration of an acid character. The sweatings occurred irregularly, three or four times in the twenty-four hours. It very soon became apparent that the disease did not partake of the nature of acute rheumatism. The right hip became greatly swollen, and a puffiness appeared over the left sterno-clavicular articulation, which became distended with fluid, but was not the seat of pain. A distinct, but very deep fluctuation having been detected in the upper part of the thigh, an incision was made, and the knife passed down through the muscles, till it reached the bone, below the trochanter; upon withdrawing it, half a pint of purulent matter escaped. The relief was great and instantaneous; the intense and tensive pain subsided; the pulse fell to 100; the skin became cooler, and the boy fell into a calm and refreshing slumber, the first he had enjoyed since the beginning of his illness, and hopes were

entertained that all might yet be well. These, however, were soon dissipated, for after very few days, this poor boy suddenly began to sink, his pulse became exceedingly weak, and he died, exhausted, apparently, by the intensity of the suffering which he had gone through, and the constitutional shock produced by the rapid death of so large a mass of osseous matter, combined with the disorganization of the hip-joint.

The periosteum was found extensively detached from the femur, and marked with patches of recently effused lymph. The denuded bone was superficially eroded, and presented the peculiar pallid aspect characteristic of dead osseous tissue. The layer of cartilage which unites its epiphysis with the great trochanter was absorbed, so that the epiphysis had become loose and detached, a circumstance which explained the occurrence of a grating sensation ("bony crepitus"), noticed in this region, the day before the boy's death. A similar cause, namely, the death of the intervening layer of cartilage, had effected the separation of the head of the femur from the cervix. The articulation contained purulent matter, and all its remaining structures were extremely vascular. The articular cartilage had altogether disappeared, that of the acetabulum had also been removed throughout the greater part of its extent.

It was interesting to consider why the articular cartilage should perish and disappear so rapidly. In his opinion it was to be ascribed to the circumstance of its being fed from the osseous surface on which it rested, and not from the synovial tissues, as was formerly supposed. Therefore when the bone died, the cartilage also perished. When the disease was chronic in the bone, it was chronic also in the cartilage, but when, as in this instance, the destruction of the bone was rapid, the destruction of the cartilage was likewise rapid.

Many specimens of this remarkable disease had been, from time to time, brought before the Society, and last year he had, himself, exhibited examples of it, affecting the femur, humerus, &c., and, at first sight, it seemed a curious circumstance that in some cases the joints were implicated, while in others they remained unaffected. This, in his opinion, depended not on any accidental circumstance; but on the difference in the normal relations of the epiphysary cartilages to the neighbouring joints. In the humerus, for example, the junction of the upper epiphysis with the shaft is completely without the capsular ligament of the shoulder-joint, whilst the junction of the lower epiphysis with the shaft is within the ligamentous connexions of the elbow-joint, and is actually in contact with the reflexion of the synovial membrane of the articulation. Acute osteitis, which usually extends to one or other of the epiphyses, and not unfrequently affects the whole continuity of the shaft of a long bone from one epiphysis to the other, will therefore necessarily involve in destructive inflammation the elbow-joint, whilst the shoulder-joint will probably remain intact. In the case of the femur these conditions are reversed—the junction of

the upper epiphysis with the shaft is far within the hip-joint, whilst that of the lower epiphysis is remote from the articulation of the knee. In acute osteitis of the upper end of the shaft of the femur, the hip-joint must therefore necessarily be involved, if, as is generally the case, the disease extends as far as the nearest epiphysis, whilst in acute osteitis of the lower end of the shaft of the femur, the knee-joint is not thus necessarily implicated. It follows, therefore, from these observations that in acute osteitis of the humerus, the prognosis is more unfavourable when the lower end of the shaft of the bone is affected, whilst, when this disease involves the femur, the prognosis should be more unfavourable when the upper end of the shaft of the bone is the starting point of the disease.—*December 18, 1869.*

Perforation of Intestine.—DR. GORDON exhibited a specimen of perforation of a portion of intestine, caused by a lumbricus. He said, that two children had been lately admitted into the Hardwicke Hospital, under his care, from totally different parts of the city, and both affected by the same train of symptoms—a low form of fever, with a great deal of pulmonary congestion—more, however, of the character of bronchitis than of general pulmonary congestion.

With regard to one of the children, a large lumbricus travelled upwards, and came out from its mouth, after which the child rapidly became convalescent. With respect to the other, who was lying in a contiguous bed, it is remarkable that the same circumstance happened. A large lumbricus came up through the œsophagus, and escaped by the mouth; but the same benefit did not result as in the other case, for the fever did not subside, and the bronchitis still progressed; but there was nothing to cause serious alarm for a few days, when, one morning, Dr. Gordon was struck with the low, prostrate condition of the child—a condition which was marked by extreme debility and fainting fits, and which ended fatally in about thirty hours.

Upon examination after death, Dr. Gordon was surprised to find that the child had died with peritoneal inflammation. It was not to a great extent, or of an acute character, but there was sufficient evidence to show that a low form of peritonitis had existed. A careful examination as to the cause of this inflammation led to the discovery that the intestine had been perforated by a lumbricus. The inflammatory action was most intense in the immediate vicinity of the perforation.—*January 8, 1870.*

Fibrous Tumour.—DR. M'DONNELL wished to exhibit a rather remarkable specimen of a fibrous tumour, 2 lbs. in weight, which was removed from a healthy young woman, twenty years of age, who was admitted into Steevens' Hospital, on the 23rd of July. On the following day he removed the tumour which was situated over the gluteal muscles.

It had been growing for three years from the time she first noticed it. It was free from pain, and the principal inconvenience she suffered was from the weight. The tumour was of extreme density; it was as hard as leather; and although it had been in spirits for some time it had really undergone no change at all. There were very large vessels running over its surface, so that on looking at it, it had the appearance they were familiar with in malignant disease. They could still see the tracks of the veins on the surface, and on a section being made large channels could be seen permeating the dense structure. This was a typical specimen of the morbid growth, called fibrous tumour, and he had had some microscopic slides prepared which showed the nature of the structure. These tumours were not at all unusual in the uterus, but there they were frequently found blended up and mixed with muscular fibres. It was one of the few tumours in which they found muscular fibres developed as a new formation. In this there was nothing but fibrous tissue; it was in a stage which Virchow would call the stage of maturity. It had begun in an exudation, composed of those minute lymphoid bodies which are alike in all these tumours in the first stage. Out of this nidus or protoplasm there comes a kind of growth which is to determine what the tumour is, just as in the development of the various tissues of the body out of the primary protoplasm. If they took a very small fetus or a chick in ovo and examined its tissues in an early stage, they found muscles, brain, bones, were all alike; they were composed of minute round cells, out of which in one case came muscular tissue; in another cartilaginous tissue; and in another bone tissue; each of them springing from an original nidus, went on to its ultimate destination, until bone, cartilage and muscle were formed, and these develop, until the stage of maturity arrives, when they undergo a gradual process of decay. In the same way tumours all began alike, whether cancer in the lip produced by the irritation of a clay pipe, or chancre on the prepuce caused by the syphilitic poison. They began in protoplasms, in any one of which the microscope could detect nothing more than was found in the other. As they go on in their life history, there comes the differentiation; they grew into the epithelial growth of cancer, or the cellular growth of syphilis, or the fibrous tissue of a fibrous tumour, and so on. This was one in which the fibroid element existed; it was one of the simplest kinds of tumour, and although they could not say from microscopic examination that it would not return, yet they knew, as a matter of fact, that tumours of that kind very rarely do return, and they might consider the patient from whom this tumour had been removed, to be nearly as safe as a person in whom such a tumour never occurred.—*January 8, 1870.*

Epithelioma.—DR. M'DONNELL exhibited a drawing representing a case of epithelial cancer on the back of the hand. The patient, a man aged

forty-seven, was admitted to Steevens' Hospital on the 27th of last July. The history of the case was this:—Seven years before a small wart came on the back of his hand. He picked this and tried to have it roughly removed, but from that time, seven years ago, until he presented himself at the hospital, it had never healed, but went on gradually increasing, until it attained the size and appearance represented in the drawing. It formed a large fungoid ulcer, springing up to a considerable height, and accompanied by a very fetid discharge. Here and there, where a probe could be passed in, it went to a great depth, and it presented the characters they were all familiar with, as those of a warty ulcer. It did not appear that there had been any thing existing of the nature of a cicatrix. The vessels of the arm were greatly dilated, and at the bend of the elbow there was an enlarged gland, and in the axilla a bunch of enlarged glands. Nothing could be done except to operate, although it was a most unfavourable case in consequence of the state of the glands, but as they were capable of being removed, it was thought that the removal of the hand and the glands in a case, the progress of which had been so slow, would give some relief from suffering, and a chance of prolonged life. Accordingly the hand was removed, at the wrist-joint, on the 31st of July, and the palm, being apparently healthy, was brought round so as to make the flap; the vessels were secured by torsion. The stump healed up with great rapidity. The glands were removed from the bend of the arm and the axilla. The interesting part of the case was the microscopic examination of the structure of the tumour itself, and of the glands. The sections made of the tumour itself showed the ordinary structure of genuine epithelial cancer, large epithelial cells deposited in loculi. It was interesting to have an opportunity of examining the glands in this form of malignant disease, which spread so very slowly, contaminated the glandular system so gradually, and was characterized by these large epithelial cells. The sections from the superficial part of the gland gave no evidence of the malignant structure, but as the sections went deeper, and deeper, the epithelial cells became more numerous, until in the centre of the gland the loculi were crammed with them, and their absolute identity with the structure of the tumour itself became apparent. The point of chief interest was that all the superficial part of the structure of the gland, its capsule and its areolar tissue, gave no evidence of any kind of contamination. The part of the gland in which the epithelial cells were found existing, and presenting all the characters of epithelial cancer, was its centre, in what must be regarded as the dilated lacteal vessel itself. Dr. M'Donnell then exhibited the morbid specimen, pointing out that the disease had extended downwards, attacking every structure, even the bone. It had, he said, a very close resemblance to what Professor R. W. Smith had described as the warty ulcer of cicatrices of Marjolin. This was, in fact, a typical case of the slow growing form of epithelial disease.—*January 15, 1870.*

Urinary Calculus.—DR. M'DONNELL exhibited a calculus which he removed on the 31st of last July, from the urethra of a patient in Steevens' Hospital. It was the largest calculus which he had ever seen removed from the urethra. The patient was a constable of police, forty-seven years of age, and was admitted into the hospital on the 14th of July, suffering from symptoms of stricture. His story was, that he had stricture for seven or eight years; that it gradually became closer and closer, until at last he was only able to make water with extreme difficulty. On examining him an exceedingly hard tumour was found in the perineum, just in front of the anus. From the history of the case he did not, at first, suspect that it was a calculus, although he said it felt as hard as a stone. They knew, however, that perineal abscesses, bound down by the strong fascia, were very often remarkable for their extreme degree of hardness. The man stated that he had long been labouring under stricture, and there was nothing in the history to lead to the suspicion that there was a calculus impacted there in the urethra.

Upon trying to introduce an instrument he found there was an impassable stricture, as he thought, but some time afterwards he introduced a fine probe, and was able to get through the stricture, and touched the stone. The removal of it was determined upon, and the operation was performed on the 31st of July. The operation itself presented some points of interest. The patient was placed on the operation table, the stone cut down upon, and removed without difficulty. He then tried to introduce, through the urethra, Holt's dilator, but it came to a stop at the stricture. He then introduced a long probe-pointed director, and this went through the stricture and dilated it so much that he was able to introduce Holt's instrument, bringing it out at the perineum, and he burst the stricture in that way. An instrument was then introduced into the bladder. Everything went on apparently very well for some days. He had no distress from the presence of the instrument, and the bladder was readily emptied from time to time. Suddenly, however, he was attacked with symptoms of acute pyemia, and died on the 24th of August. Upon examination after death, they found a very thickened bladder, as thick almost as the walls of the heart. The posterior part of the urethra was perfectly healthy. They now came to the dilated part, where the calculus was impacted. There was a small opening in the perineum; it was clean and healthy-looking. The seat of the stricture was immediately in front of this opening. The portion that was cleft by the dilator was the submucous tissue alone; the splitting did not go through to the erectile tissue, neither was there any infiltration of purulent matter. He laid stress upon this, for he thought that all those operations which injured the erectile tissue, or spongy portion of the penis, were likely to be followed by pyemia. The only case he knew of, that had proved fatal after Holt's operation, was one in which the erectile tissue had been

injured. He believed that pyemia arose in this case from the fact, that the old standing disease of the bladder and urethra had given rise to that morbid condition which predisposed to the disease. With regard to the calculus, it was evident there had been a much smaller one, of an almond shape, that had escaped originally from the bladder edgeways, and when meeting with the stricture, it could not pass any further, and remained impacted in that situation, where it gradually increased until it attained its present magnitude.—*January 15, 1870.*

Rupture of the Heart.—DR. HAWTREY BENSON gave the following account of the case of a rupture of the heart:—We have before us, Mr. President, the heart of an intimate and valued friend of my own, and one well known to you, Sir, and to the profession at large. He was a man most distinguished in his profession, and his name has long since reached far beyond the limits of these islands. He was 64 years of age, and yet, up to the last, when in health, he was particularly fond of long country walks, and for the last eight or ten years I have been his most constant companion. On several of those occasions, when ascending a rising ground or hill in the course of our walks, he suffered very considerable attacks of dyspnea, though his lungs were quite healthy. He frequently complained of headache, or pains through his arms, but never of any uneasiness referred to the heart. About this time last year, my friend, Dr. Wharton, and my father and I, were attending him during an illness, and were struck by the extreme feebleness of the action of the heart, and expressed our opinion that he had a weak, fatty heart. However, he appeared in good health up to some weeks ago. A few days before his death I had a letter from him, saying he was not well, and was confined to bed. I found him suffering from a slight feverish cold; on the next day he was much better, and the third day he was out driving, and visited several of his patients. At one of his visits, one of the inmates of the house said to him, "You are not looking well." He replied, "I am not well—I am sure I shall soon die." (He made the same remark to one of his colleagues, in a letter, three days previously). A few minutes after that conversation he suffered from intense mental excitement. He seemed to feel quite faint; he sat down in a state of exhaustion, and put his hand to his head, and called for a glass of water. He soon rallied, however, and went home. That evening he appeared in his usual health and spirits, ate a hearty dinner, and went to rest at his usual hour. On the next morning he breakfasted in bed, at his wife's request. He then got up, ordered his brougham, shaved himself, and was about to take a cold bath, which, ever since boyhood, he had been accustomed to take in the morning. He threw off his shirt, and stepped into one of those large, shallow, circular baths. He dashed a spongeful of water on his chest, and immediately exclaimed, "I am

going to faint," and fell on the floor. His man, who was in the room, rushed to him, but found not the least appearance of life in him. His wife sent at once for medical assistance; and the first who arrived on the scene was my father, and soon afterwards, Mr. Adams, but before either reached the house, all was over.

A *post mortem* examination was made seventy-two hours after death. There was a great deal of livor about the back and upper part of the neck. We found the lungs remarkably healthy, but the pericardium was seen distended to the utmost; and on opening it, twelve ounces of black, clotted blood were found collected in its cavity. We next searched for some cause to explain the presence of this blood. We could see no aneurism, but on turning up the heart, a patch of ecchymosis on the posterior wall of the left ventricle attracted my attention, and in the centre of this I discovered a rent, three quarters of an inch in length. It occurred in the longitudinal axis of the heart, and parallel to the superficial muscular fibres. Midway between the extremities, however, the rent had taken a transverse direction for about one-eighth of an inch, so that the fibres did not appear merely separated, as at the extremities, but were here torn across. When the ventricle was opened, we found that the rent internally was concealed by a few very strong muscoli papillares. The rent was extended to the very point of insertion of these, so that its inferior extremity corresponds exactly to the acute angle formed by the convergence of those muscoli papillares and the posterior wall of the ventricle. On this inner side the length and character of the rupture is very much the same as it is externally—it is, perhaps, slightly longer. The valves are all healthy. There is a very slight degree of calcification of the left zona tendinosa, and a slight unevenness, caused by commencing atheromatous degeneration on the inner aspect of the aorta. The heart is of a fatty, soft character, drab-coloured, and quite friable; and under the microscope it presented all the appearances of fatty degeneration. My friend, Dr. Macalister, also kindly made a microscopic examination of it, and writes:—"The specimen is a very characteristic one of fatty degeneration. I have rarely seen one more distinct. It presents all the typical appearances which distinguish the true 'fatty heart.'" One thing which struck us was the somewhat unusual position of the rent at the back of the ventricle, instead of the front. Could the insertion of these muscoli papillares have had anything to do with the occurrence of the rupture, or with determining its exact position? Death, by such a rupture as this, may be looked upon as a rare event, when we consider that Harvey, who was the first to observe any case of this kind, recorded but one case. Baillie, in his long experience, met but with one instance. Corvisart tells us himself that he never met with one. Dr. Stokes, in his work on "Diseases of the Heart and Aorta," makes no mention of any case

having come under his own immediate observation ; neither do Bellingham nor Mr. Adams. The latter, in his publication on "Diseases of the Heart," in Vol. IV. of the "Dublin Hospital Reports," mentions two cases which occurred in the practice of two of his friends, Dr. Cheyne and Professor Colles. One of these hearts is now in the College of Surgeons, and it is the only specimen of the kind in the Museum. My father, too, and Dr. Law, both of whom have had such long experience, inform me that they have never met with a case in their own immediate practice. Several continental observers have compiled a number of cases of rupture of the heart ; but the instances I have given are sufficient to show that, even with men who devote a life-long attention to the subject of heart disease, the number of cases which fall to the lot of any one observer is very small indeed. As to the cause of rupture in the case before us—this gentleman was particularly active in mind and body. He was accustomed to great physical exertions ; and at times, to very considerable mental emotions and anxieties ; and it is hard to suppose that, having escaped all these sources of danger, the mere dashing of water on the chest was the *sole* exciting cause of rupture. It appears much more likely, that on the day previous to his death, when he suffered that unusually severe mental excitement of which I spoke, the heart did not wholly escape injury, but that some of the inner fibres of the ventricle gave way at the time, accounting for the temporary faintness he experienced ; and that it was not till next day, when he was subjected to the shock of the cold water, that the remaining fibres ruptured with fatal result. If that were the case, however, we should expect that it would be followed by great derangement of the heart's action ; but there is no evidence of that having occurred. Yet, might not the absence of great derangement be accounted for by the altered condition of the muscular tissue of the heart ?

The absence of marked pain would, in that case, argue the low animal sensibility of the muscular structure of the heart in cases of fatty degeneration of that organ.—*January 15, 1870.*

Ovum in a Case of Blighted Embryo.—DR. M'CLINTOCK said the ovum or part of the ovum may be retained in the uterus for several weeks or months after the embryo has been blighted, and this fact, if overlooked or disregarded, may occasionally place a practitioner in an exceedingly embarrassing position. The specimen, which I now exhibit, is a very striking illustration of this prolonged retention of a blighted embryo. Any one familiar with the examination of the human ovum would say that this was a conception of five or six weeks. The little embryo, very perfect, and not much larger than a house fly, can be seen hanging by the umbilical cord ; a casual observer would naturally say this conception took place six weeks or two months ago. The fact is, however, that in

this case conception took place seven months ago, and it is this which imparts to the case its clinical interest. The lady had had several children previously, and so ought to have been tolerably familiar with the subjective phenomena of pregnancy. She had been carrying this ovum for six months; but of course, the simple explanation is, that it was blighted at the end of six weeks from the time of conception, and retained. There is another fact connected with cases of this kind, that while all vital connexion between the ovum and the uterus has ceased, the ovum may be retained in the uterus subsequently, and finally expelled in a non-putrefactive state, which is a very curious and inexplicable circumstance. This is a good illustration of it; for although there is no doubt the vitality of this ovum had ceased several months ago, it was not expelled putrid, nor was there any offensive odour connected with it. For the last three or four months the lady had been greatly tormented with hemorrhage. When I was asked to see her, and was told of the seven months' pregnancy, and of the hemorrhage, I began to think the case was one of placenta previa; but a very superficial examination was enough to show how erroneous this supposition was. The abdomen was flat and resonant all over, and on instituting the requisite examination internally I found an ovum partly extruding from the uterus; it was removed easily; whereupon the hemorrhage ceased. The retention of this had given rise to some irritative fever; for the lady had occasional rigors; the pulse was frequent; and her skin was dry and hot. I have before alluded to considerations of a medico-legal nature connected with such cases as these. The size and development of the ovum would be no proof of the time at which conception took place; hence if a woman some months separated from her husband, expelled such an ovum as this, and a practitioner were to say that it was only a conception of a few weeks, he would cast an imputation on the woman's character that might involve him in disagreeable consequences. Such a case as this therefore shows the necessity of great caution. Dr. Mathews Duncan, of Edinburgh, has described a case in which an ovum of a similar kind was retained seven months in utero, the lady having in the meantime performed a voyage from India to Edinburgh, without the ovum being expelled. In this case there was no trace of an embryo; to use the expression applied to the eggs of hens, and which is quite appropriate here, it was truly an addled ovum.—*January 22, 1870.*

Urinary Calculus.—DR. BARTON exhibited a calculus which had a hair-pin as its nucleus. The following was the history connected with the specimen:—A woman, aged twenty-eight, married, and the mother of several children, introduced this hair-pin into her bladder upon the 3rd of June, 1869. She was admitted into the hospital in November with irritation of the bladder, evidently arising from the presence of some

foreign body. A sound being introduced it was felt distinctly. The bladder was so contracted that it held only three ounces of urine. The urine was loaded with a quantity of ropy mucous filled with ammoniacal deposits. The urethra was dilated, and the foreign body brought into the long axis of the bladder, towards the urethra, and extracted. It consisted of a deposit of phosphate of lime and triple phosphates formed into a calculus, part of which was soft and readily broken down, but some portion of it was very hard. The mass of the deposit was formed along the head of the pin, which was more lightly coated lower down. It weighed eighty-six grains. Subsequently the bladder was washed out carefully, and the woman left hospital perfectly well. I examined the bladder very carefully before she left, and satisfied myself that it contained no remnant of the phosphatic deposit.—*January 22, 1870.*

Phlebitis of the Cerebral Sinuses—Disease of the Tympanum.—**DR. STOKES** said the patient from whose body these specimens were taken was forty-nine years of age, a fine muscular and regular living man; by trade a fisherman. He had been subject to chronic otorrhea for years. It occurred periodically, coming on with headaches, and ending by profuse purulent discharge, frequently offensive, from the ear. He was deaf, particularly so on the right side. We never ascertained whether he was absolutely deaf on this side or not, but it was probable he was, inasmuch as on dissection the tympanum was found destroyed, and there were marks of chronic disease in the bone. He was admitted to hospital on the 12th of January last, and was then complaining of what seemed to be simply an attack of ordinary capillary bronchitis from cold. He had no fever whatever; his pulse was 65, and perfectly regular; the action of the heart perfectly normal, and no one could discover from the pulse or from the action of the heart that there was any serious malady about him whatever, nor up to the time of his death, which took place on the night of the 25th, was there any febrile disturbance whatever. The second day after his admission I was surprised to hear that the man had been delirious in the night, but the delirium had passed away, and during my visit he was perfectly calm. This nocturnal delirium continued. It was of a mild character. He was never violent, and whenever I spoke to him he always, until the last days of his life, returned a perfectly rational answer; and it was observed that at night he would become gradually more and more unintelligible. On the morning of the 17th of January we were surprised to find great tumefaction of both upper eyelids, and there was manifest over them a slight erysipelatous redness.

There was hardly any vascularity of the conjunctiva; no pain whatever was complained of, and there was no intolerance of light. This remarkable symptom subsided in the course of forty-eight hours, completely so in one eye, and almost completely in the other. Then he got

a slight degree of subsultus of both arms, and this continued to increase. Then picking of the bed clothes occurred. On the 23rd of January he was examined carefully to see if there was anything like puffiness or œdema in the region of the temporal bones or of the mastoid process. Mr. White saw him in consultation, and he recognized a very slight degree of fulness behind the right ear; but we could not say there was any œdema. Taking the history of the case into consideration it was determined to cut down over the mastoid process. This was done but without any discharge of pus following. On the next day an opening was made into the mastoid cells with a semicircular saw, but there was no discharge of pus. The delirium became permanent; the man became incoherent, then insensible, and died.

The most remarkable symptom that occurred during the progress of the case was the swelling of the eyelids. I am not aware that it has been noticed in any of the cases that have been brought before us. It was one of the first circumstances that led me to believe that the patient was labouring under purulent poisoning. The spontaneity of the attack in both eyes, and the absence of the vital symptoms of inflammation, appeared to indicate that it was a result of poisoning of the bloody purulent matter. Dr. Bright in his essay on arachnitis of the base of the brain has noticed this particular symptom.

Dr. Foot made a careful *post mortem* examination, and has kindly favoured me with the following account of the results:—

The lining membrane of the external ear was thickened, whitish, and sodden, from long contact with purulent matter. The membrana tympani was destroyed, with the exception of a narrow rim of its attached border, and the malleus was, in consequence, set free. The mucous membrane of the middle ear was pulpy, vascular, and bathed in pus; more particularly that portion of it which lines the inner wall. The mastoid cells opening into the tympanum had their lining membrane in the same condition as that of the middle ear. The lateral sinus was completely obstructed by softening coagula, and purulent matter lay between the outer wall of the sinus and the bone on which it rested. The superior and inferior petrosal sinuses were occupied by similar clots, and the cavernous sinus was likewise the seat of a suppurative phlebitis. The parts of the dura mater related to the petrous portion of the temporal bone were thickened, easily detached, and, in many places, actually separated by lymph from the bone beneath. It appeared from the absence of fœtor, and of discolouration or softening of the bone, that the lesion which caused death was essentially, not a caries, but a suppurative phlebitis of the sinuses of the skull, propagated through the cells of the mastoid bone from the inflamed mucous membrane of the middle ear. It would further seem that the peculiar ocular phenomena, which suddenly appeared a few days before death, were due to the obstruction of the right cavernous

sinus, which interfered directly with the venous circulation of the right eye, and indirectly with that of the left.—*January 29, 1870.*

Microphthalmos.—MR. WILSON said, the specimen he had to exhibit was an example of a congenital malformation, which was, no doubt, known to the members of the Society by name, but which had not, he thought, hitherto been exhibited at their meetings. It presented the appearance denominated microphthalmos, or small eye. He removed it on the Wednesday previous from a boy aged nineteen. The antero-posterior diameter measured $7\frac{1}{4}$ lines—the lateral diameter, 8 lines. The cornea was extremely small, measuring only 2 lines in its transverse axis; it was transparent during life, and allowed of the iris being seen and a coloboma in it.

During life there was a metallic reflection from the bottom of this eye, somewhat similar to that seen with the ophthalmoscope. This could be obtained at any angle of incidence. At the time, he considered it was either due to lymph being effused in the bottom of the eye, or to malignant disease, as it was known that fungus hematodes, though occurring, as a rule, only in childhood, gave this peculiar reflection. On removing the eyeball, he found, however, what was, he believed, the true explanation. It was due to the same cause as that which affected the eye of the albino. In the eye of the albino the pigment was very scanty, the iris and choroid scarcely pigmented at all; the light therefore entered at all sides—through the iris, sclerotic, and choroid. In this instance the sclerotic was extremely thin, so much so, as to be perfectly translucent, and, he believed, the pigment of the choroid was almost totally absent. The peculiarities of the case were the measurements. The measurements approached those of most animals, the lateral diameter being greater than the antero-posterior diameter. Attached to the posterior part of the globe, there was a tumour which he had been able, with some difficulty, to separate from the optic nerve-sheath inferiorly, but at the upper and outer side, it was intimately attached to it, and to the sclerotic. It was composed of fibrous tissue and presented several cyst-like cavities filled with an oily fluid. The largest axis of the tumour was 4 lines. The present case was one of arrest of development, and was to be distinguished from one of atrophy of the globe.

The case afforded also another feature of interest, for the coloboma of the iris enabled us to fix the period of gestation in which this arrest of development occurred.—*January 29, 1870.*

Pericarditis.—DR. M'SWINEY said, the morbid specimen which he exhibited, had been taken from the body of a patient who died in Jervis-street hospital last week, suffering from the effects of inflammation of the pericardium and endocarditis, consequent on acute articular rheumatism.

His history was, that he had an attack of rheumatism four years previously, which engaged his larger joints, and that he was under Dr. Head's care while suffering from it. He also stated that two of his brothers died from inflammation of the heart in connexion with articular rheumatism; and that an uncle had suffered very severely from two or three attacks, and had been a patient of Dr. Law's. Dr. M'Swiney's patient had been three days ill. His joints were comparatively but slightly affected, but there was a great deal of high fever. He was totally free from cardiac or precordial pain. On auscultating the precordial region, Dr. M'Swiney made out the existence of a soft systolic basic *bruit*; and taking the man's previous history into account, he came to the conclusion—although he was somewhat doubtful about it—that this must be consequent on a former attack of rheumatism engaging the aortic valves. The rheumatic attack pursued its usual course; and on the third day after reception into Hospital there was a loud, double soft *frottement*, a friction murmur audible all over the region of the heart. On the fifth day this disappeared; and the condition of affairs now was, that the normal area of precordial dulness was most considerably enlarged. On the ninth day there was the friction murmur again audible over a limited space of the heart's area; and on the tenth day he died. Here it is necessary to relate that during the progress of his acute attack he had an attack of pleuritis; and again, on the fifth day the base of each lung was found absolutely dull on percussion, and nothing but a fine mucocrepitating *râle* was audible in these situations. Believing that this indicated the presence of an attack of pneumonia (an affection which is not uncommon in connexion with the heart's inflammation), Dr. M'Swiney used as an assistant test a means of diagnosis described by Todd and Beale—namely, the ascertainment of the absence or presence of chlorides in the urine, by means of nitrate of silver. He found, however, no trace of their presence in this instance, thus confirming the diagnosis of pneumonia. There was no expectoration, but the patient suffered from a hard, and distressing, dry cough. He died suddenly, comparatively speaking; and upon making a *post mortem* twenty hours afterwards, the heart was found to present a very excellent specimen of the effects of the exudation of lymph in connexion with inflammation of the pericardium. The entire surface of the heart was covered with ridges of lymph, and the corresponding surface of the pericardium was invested by a thick mass of reticulated lymph, which was adherent in parts, and which, when separated, presented the appearance shown by the surface of a piece of cut sponge; whilst between the heart and the pericardium there were fibrinous prolongations, which, had the patient lived, subsequently would have formed permanent and organized adhesions. On looking to the interior of the heart, the mitral valve was found to have recent lymph deposited upon it in small quantities, and in the aorta at one point there was also a

fibrinous deposition of a very consistent character. On the outer surface and edge of the sigmoid valves, lymph deposit was also to be observed. Dr. M'Swiney observed that this case afforded an example of a patient who had manifestly an inherited tendency to the rheumatic diathesis. It was also an instance of two other diseases, pleuritis and pneumonia, arising in the progress of a case of pericarditis. The lungs were adherent all round to the parietes of the chest, and on both sides were in a state of absolute consolidation. Again, the absence of the chlorides was a noteworthy fact in connexion with the existence of the pneumonia. Finally, it was noticed that the uncut pericardium was much distended; but on opening it but a small amount of fluid was found, whilst an escape of the most fetid air took place—an occurrence extremely rare, although it has been sometimes described, where there had not been any putrefaction or ichorous secretion present in the pericardium to account for its production.—*January 29, 1870.*

Aneurism of the Aorta.—PROFESSOR LAW exhibited an aneurism of the ascending aorta, with hypertrophy of the left ventricle—the subject, a man aged forty, a labourer. He had always enjoyed good health, with the exception of having felt flying pains through his chest, especially under the left nipple, in 1854. In 1868 he was treated in hospital for retention of urine, and was completely relieved. He now continued well till November, 1869; then he said he felt a lump in the epigastrium, and a pain there when he made a full inspiration. His breathing now gradually became embarrassed, his feet and legs began to swell, and the swelling increased till he was admitted into Sir Patrick Dun's Hospital, January 18, 1870. He now chiefly complained of most urgent distress of breathing, and stiffness of his lower limbs and body. He had almost general anasarca. The legs were much swollen, and hard—did not pit much on pressure. There was considerable effusion into the abdomen. The liver was felt much below the right hypochondrium. The face was congested and swollen, and had a slightly jaundiced hue. There was considerable effusion of serum, mixed with blood, into the sub-cellular tissue beneath each conjunctiva. The heart beat with a strong impulse. A loud, rough, double murmur was heard behind the upper bone of the sternum, where the second rib joins this bone at the right side. This murmur was heard along the course of the large arteries of the neck. The lungs were very much congested. There was some, but not much dulness to percussion in the precordial region. There was considerable dulness on both sides of the chest posteriorly. Pulse was jerking, collapsing, and visible at the wrist. Urine was scanty and high-coloured, but contained no albumen. He had orthopnoea. Although he complained most of his distress of breathing, yet he was scarcely less loud in his complaint of the great discomfort he experienced in his entire body, from the sense of fulness and

weight. He had cough, with expectoration, which was occasionally tinged with blood. The dropsical swellings gradually increased. The hands and arms were the last to become œdematous. The pulse became more feeble, the impulse of the heart weaker, and the cardiac murmur less distinct. There was more blood in the expectoration. There was almost complete suppression of urine before his death, which took place twelve days from his admission into hospital. His wife asserted confidently that he worked, and actively, as a labourer, until five weeks before he was received into hospital.

Post mortem examination exhibited the following appearances:—An intimate and tolerably firm adhesion between the two pleuræ in their entire extent, in each cavity of the chest. There was also a close adhesion between the exopericardium and the pleura lining the internal surface of the left lung. Both lungs were in a state of extreme congestion. The cavity of the pericardium was obliterated. The two laminæ did not adhere so firmly but that they could be separated. The fine cellular structure that united them was stained with bile. The heart was much increased in size from hypertrophy, with much augmented capacity of its left ventricle. The whole thoracic aorta was a rigid tube, which, when slit open, exhibited deposit of bone in different parts, especially near the heart. From the posterior part of the ascending arch, at the very commencement of the artery, there arose an aneurism, about the size of an orange, which communicated with the artery by a round opening with thickened edges, about the size of a half-crown piece. This opening was so immediately connected with the valves as to render them incompetent, so that they did not sustain a column of water. The opening from the artery into the aneurism was immediately above the right and posterior sinus of the aorta. The tumour projected forwards and downwards, and to the right side, so as to involve the right auricle. It compressed the vena cava descendens backwards against the root of the right lung. This pressure it exercised upon the vein from the point of entry of the azygos vein to its termination in the auricle, and so reduced its calibre that a No. 10 catheter filled it completely. The tumour projected so far into the right auricle as to conceal the orifice of the vena cava ascendens from the front view, although it did not absolutely press upon its opening. The aneurism was strictly intra-pericardial.

There was a considerable effusion of serum deeply tinged with bile into the cavity of the abdomen; the liver and both kidneys were very large, and very congested. There was a large quantity of blood effused into the cellular structure behind each kidney.

Professor Law confessed he had some difficulty in reconciling the condition of the patient with the date to which he referred the commencement of his illness. He could hardly believe it possible that five weeks previously he could have been employed in hard labour, which Dr. Law

had been assured was the case. His first impression was, that it was a case of acute dropsy occurring in a man already affected with valvular disease of the heart, in which event he expected to find albumen in the urine, but none was found in it. It was the cause of the great extent of dropsy that was difficult to understand, but which the *post mortem* examination fully explained. It was evidently due to the pressure of the aneurism on the cava descendens, which accounted for the congestion and œdema of the upper parts of the body, and the sero-sanguineous effusion into the sub-conjunctival cellular tissue; and to its projecting into the auricle so as to prevent the free passage of the blood from the cava ascendens into the auricle. Thus, in fact, was there congestion of all the venous system. Hence followed the dropsical effusions of blood and serum met with in different parts, as also the congestion of the different organs.

Dr. Law observed:—From no part of the thoracic aorta could an aneurism arise where it is less likely to betray itself by symptoms arising out of its connexions than from the posterior part of the ascending arch. There are fewer parts there with which it may be connected; and when, as in the present case, it involves the valves, it becomes more difficult of detection from its assuming the characters of valvular disease, and sheltering itself under its signs. It had often been observed how rarely thoracic aneurism is attended with murmur, and that the exceptional case was when the aneurism involved the valves. Dr. Law adverted to the congestion of the different organs, and the consequences of such congestion, as the jaundice, and hæmoptysis, as also the effusion of serum mixed with blood into the sub-conjunctival cellular structure, which he found difficult to account for till the *post mortem* examination explained it, as also the effusion met with in different parts. Although the aortic valves were thickened, yet no doubt their incompetency was mainly due to their intimate relation with the aneurism, which pressed upon them in such a way as to effectually hinder their action. Dr. Law remarked upon the universally admitted difficulty of the diagnosis of thoracic aneurism, which, when lying in the depths of the chest, owed its detection most frequently to the symptoms produced by its interfering with the functions of different organs with which it came in contact in the course of its development; that these organs were those of respiration, deglutition, and the nerves. It might press upon any portion of the respiratory apparatus within the chest; either on any part of the pulmonary parenchyma, and so modify the respiration in the part so pressed; or on the trachea, and produce stridulous breathing; on either bronchus, and render the respiration in the corresponding lung comparatively feeble; or it might press directly on the œsophagus, when it engaged the descending aorta, and then cause dysphagia; or it might press on the recurrent nerve, and thus cause atrophy of the laryngeal muscles, a specimen of which Dr. Banks produced at a meeting of the Pathological Society, some time since. Dr. Law

considered these cases always interesting, but especially so just now, as intra-thoracic tumours engaged a good deal of attention.—*February 5, 1870.*

Aortic Valve Disease.—DR. LITTLE said, the reasons he wished to exhibit the heart and lungs now before the meeting were, first because there had been an error committed in localizing the exact seat of the disease, and they were able, after death, to discover the cause of the mistake; and, secondly, because the case illustrated a condition which sometimes occurred in valvular disease of the heart, and was the immediate cause of death—a condition which had been often described, and the possible supervention of which should never be forgotten; but which in this case had not been sufficiently present to his mind at the bed-side, or he probably might have been able to prolong the patient's life for a short time.

The woman had been under his observation for one year. She was between fifty and sixty years of age, and for twenty years had cough and shortness of breathing. For five or six years she had experienced an unpleasant feeling about the heart. She had been a few weeks confined to bed—the symptoms being, cough, shortness of breathing, expectoration, oppression in the chest, and a distressing feeling of fulness in the abdomen. There were indications of great venous congestion, puffiness of the feet, distention of the jugular veins, and fulness of the rectum, owing to the engorgement of the hemorrhoidal veins. On one occasion there was blood in the urine, and there was a red discharge from the uterus—a symptom which is often one of the first to alarm a woman who has valvular disease of the heart.

On physical examination, the lungs were found to be emphysematous; there was undue resonance over the chest, an absence of dulness over the heart, and a cardiac murmur. He arrived at the conclusion that this murmur was at the tricuspid orifice; it could be heard at the apex, but not in the axilla; it was audible over the sternum, at the level of the fourth costal cartilage, but could not be heard in the course of the innominate artery. The area it occupied extended over the lower third of the sternum and the chest walls, for an inch on either side.

There was a marked fulness, though no pulsation in the jugular veins. It was evident that the blood could not return readily to the right side of the heart. On yesterday week he took an opportunity of showing the patient to the pupils for the first time; and as a proof that the symptoms of abdominal distress were entirely due to congestion of the tributaries of the portal vein, he drew attention to the fact that he had taken some ounces of blood, by leeches, from the margin of the rectum, and thereby given great relief. On last Tuesday, the patient was sitting at the fire, and appeared to be tolerably well, when she shivered, and went to bed.

She very soon became delirious, complained of great pain in the region of the diaphragm on both sides, spat up a large quantity of blood, gradually sank into a comatose state, and died in the afternoon.

The first thing that attracted his attention, at the *post mortem* examination, was the extreme fulness of all the veins on the surface of the body, and when he made an incision into the parts, the large quantity of blood that flowed. He was greatly struck with the enlargement of the tributaries of the portal vein. On opening the thorax, the distention of the right cavities of the heart was extremely marked. The lungs were emphysematous, and in their lower lobes contained some extravasated blood. They found disease in the left side of the heart, and that it was at the aortic orifice the cause of the murmur was seated, and not at the tricuspid. The orifice was studded with calcareous matter, which must have caused a ripple in the current of the blood. The cause of error in the localization of the murmur arose from two circumstances—the heart was pushed down by the emphysematous lungs, and the great vessels were so overlapped by them that the murmur, which was doubtless present in the innominata, could not reach the ear. When he opened the left side of the heart, he noticed that the left ventricle was perfectly free from blood. It was clear that the blood did not, in any quantity, get into the left side of the heart; it had accumulated in the right side, and in the lungs; in all probability, the right ventricle had become so distended that it could not relieve itself; and it was not improbable the timely abstraction of a few ounces of blood might have prolonged life.—*February 5, 1870.*

Cancer of the Stomach and Mesentery.—DR. STOKES said, that the specimens on the table, consisting of the liver, stomach, and mesentery, were taken from the body of a young woman, who died lately in the Meath Hospital. Her history was somewhat interesting.

She had enjoyed perfect health till about nine months ago, when she was exposed to severe domestic distress, which acted very much on her entire system. She stated that she never felt well afterwards; and she perceived a hard tumour about the umbilicus. This was the first evidence there was of organic disease. She became subject to frequent pains in the stomach and abdomen, and severe pains in the back; and when she was admitted to the hospital, under the care of Dr. Hudson, there was unquestionable evidence of the presence of an organic tumour. The abdomen was full, rather rigid; and on placing the hand over the epigastrium, and over the umbilicus, it was plain that a large mass of formidable organic disease existed. The tumour was nodulated, and in parts of a stony hardness. It was movable, but only to a very slight extent.

Black vomiting came on, and the patient's countenance gradually

became strongly characteristic of cancer; but about ten days before her death it became much less marked. She died, worn out by pain and exhaustion, the vomiting continuing to the last. Towards the close of life, she found relief from lying entirely on her face. During life, Dr. Stokes observed, I thought I could distinctly feel the thin edge of the liver. Dr. Hudson had had the patient under his charge some few days before she was placed under my care; and he stated that between what I took for the edge of the liver and the real margin of that organ there was a distinct line of demarcation.

The *post mortem* examination showed that this opinion was correct, and that what I had supposed to be the margin of the liver was really that of the mesentery, loaded with cancer. The liver was perfectly normal, both in size and form.

It was found that the case was one of malignant disease extensively engaging the pyloric extremity of the stomach, as well as the mesentery and a large number of the lymphatic glands along the spine, which may, perhaps, account for the continued pains in the back.

The stomach was of prodigious thickness. The cardiac end of the organ was scarcely at all affected, but the pyloric extremity was thickened prodigiously. There was no ulceration of the interior of the stomach; the pyloric orifice was only slightly narrowed—a finger could be passed into it with facility. The liver and lungs were quite free from cancer.

As to the nature of this cancer, it seems to occupy a place midway between cephaloma and scirrhus, it being harder than the former, and softer than the latter. Dr. Foot has made a careful microscopic examination of the morbid structure, and the views I express as to the nature of the deposit are his own, and I fully agree with them.

One of the most interesting facts connected with this case was the apparently rapid course of the disease. There can be very little doubt that within a month after the patient being in perfect health, she began to suffer from the symptoms and effects of the morbid growth. It was too much the habit to connect the disease of cancer with chronicity; for there are certain conditions in which cancerous development exhibits an extraordinary rapidity—where the system for instance, is supersaturated with cancer.—*February 5, 1870.*

Pneumonia; Enlarged Kidneys.—DR. HENRY KENNEDY detailed the following case:—Wilds, a man of thirty years of age, was admitted to the Cork-street Hospital, January 17, 1870. He had drank hard, and been affected with syphilis, but his look was that of a healthy man. When first seen, he was labouring under a severe attack of pneumonia of the lower lobe of the left lung, in the second stage, marked by all the usual symptoms, and also very severe pain in the side. Under ordinary treatment, the disease yielded, and so rapidly, that by the third day it

had nearly ceased. At this period he began to suffer from vomiting (he had not taken any tartar emetic), and threw up bile, in considerable quantity, and of a very dark colour. As this went on, his tongue, which at first was coated with a white fur, rapidly changed its appearance, and became red, glazed, hacked, and, finally, blood oozed from both it and the gums. With this state the general symptoms became much more serious. The pulse was rapid and small, the face sunk, and the extremities livid and cold. Nothing whatever was retained on the stomach. He died, ten days from the time the vomiting began—his mind being wonderfully clear all through his illness.

On *post mortem* examination, the lower lobe of the left lung was found semi-solid, and of a blue colour, the appearance of lymph on it being much less than usual. This state may possibly have been due to the rapid wasting which went on during the last ten days of the patient's life. The kidneys, however, were the organs at fault; for they were found much enlarged, nearly double their ordinary size, and afforded marked specimens of the large white kidney, constituting one of the forms of Bright's disease.

This case Dr. Kennedy thought one of practical importance; for, though disease of the kidneys frequently gives rise to affections of other organs—the stomach amongst the rest—it was not common for the symptoms to assume the very acute form which they did in this instance, and which ran their course in ten days. In this latter point of view the case was one of very considerable importance.—*February 5, 1870.*

Foreign Body removed from the Urethra.—MR. STOKES exhibited a bean, which he had removed from the urethra of a boy, aged eight years, who was recently under his care in the Richmond Hospital. He came in suffering from extreme pain, and inability to pass water. His mother stated that, for five months, he had occasionally attacks of retention of urine; but under the influence of warm baths, he was able to pass water; but, on the occasion of the last attack of retention, these expedients failed entirely. About three-quarters of an inch from the orifice of the urethra, a hard body, which appeared to be a calculus, could be felt. He introduced a small forceps, and endeavoured to seize it, but failed to do so; and it was not until he enlarged the orifice of the urethra by incision, that he succeeded in extracting the supposed calculus. When he first took it out, he thought, from its rough appearance, that it was a mulberry calculus; but, when cleaned, it proved to be a bean, which the boy must have introduced himself, and which lay there for five months. The intermittent nature of the attacks of retention of urine was remarkable.—*February 5, 1870.*

Foreign Body removed from the Urethra.—MR. STOKES exhibited a strong

iron hair-pin, which he had removed recently from the urethra of a female, aged twenty-eight. The patient was admitted into the Richmond Hospital suffering from extreme pain in the region of the bladder, and inability to pass water. The bladder was found to be greatly distended, and very sensitive. On making preparations to introduce an instrument, about a quarter of an inch of the bent portion of the hair-pin was found projecting from the orifice of the urethra. An attempt was made to remove it by the forceps, but at first without success. However, by gently rotating the foreign body, and making further traction, which was attended with considerable difficulty, owing to the violent struggles of the patient, the foreign body was finally successfully extracted, without any injury being done to the mucous membrane of the urethra. The history that the patient gave was this:—She stated that she was a ladies' maid, and that some months previously, while dressing her mistress for an entertainment, she threw some hair-pins on the bed; and after her mistress left the house, she threw herself upon the bed, and fell asleep, and that she was suddenly awakened by feeling violent pain in the region of the urethra; she believed that it was at this moment the hair-pin accidentally got in. She was a person of extremely hysterical temperament, and they must reject her story. It was much more likely she herself introduced the hair-pin. He had mentioned that the bent portion projected from the external orifice of the urethra. The bent portion must have been introduced first, and the pin then passed into the cavity of the bladder, where it remained for a considerable period, and became incrustated with deposit, and was finally driven forward into the urethra along with the urine.—*February 5, 1870.*

Thrombosis.—DR. WALTER SMITH said, in the absence of any clinical history attached to the specimen he now exhibited, he could do little more than merely exhibit it to the Society. Coagulation of the blood in the vessels during life had been chiefly observed in three situations—in the veins of the lower extremities, in the veins of the true pelvis, and in those of the upper extremities; but the specimen now before them was one of thrombosis affecting the veins of the right side of the neck and right upper extremity. It was taken from the body of a male subject in the dissecting-room; and from the aspect of the subject before an examination of the viscera was made, it might be expected that he had been labouring under some form of heart disease. The face and upper portion of the body were extremely congested, and there was a considerable amount of dropsy present. Engaged in dissecting the right side of the neck, he came on what he thought at first was the carotid artery, a little more external than normal, but on looking more closely, he found it was the internal jugular vein, nearly occluded by a very firm clot. The clot, on closer examination was found to fill up only a part of the vein. It did not reach to the base of the skull; but it extended downwards so as

to intrude into the innominate vein. It also extended down the right subclavian, to the axillary and brachial veins, being more marked at the valves; small isolated thrombi were also found.

There was a small portion of clot in the left vena innominata, so adherent to the coats of the vessel that it could not be scraped away. The heart was dilated, the right auricle considerably so. The valves were all healthy except the tricuspid, which was thickened, and to which two or three small nodules were attached. There was no atheroma in the arteries, and the lungs appeared healthy. The reasons for believing the clot found in these vessels was not formed after death were its extreme firmness, distinct lamination, pale colour, and the intimate adherence of it to the coats of the vessels. The abdominal cavity was not examined, and he could not speak as to the condition of the renal veins. The clots seemed to have originated in the veins themselves. There was no evidence of phlebitis or of metastasis having taken place, nor was there any appearance of pyæmia in any portion of the body he had examined. The brain had been removed, and he had no means of ascertaining whether the clot had extended into the cerebral veins. As they did not know what the life history of the man was, it was impossible to say what the phenomena were caused by. In the absence of such knowledge, they could only speculate as to the cause of the appearances presented. At the back of the superior vena cava there was a large cluster of black, bronchial glands, matted together; and it might be that the retardation of the blood current in the veins of the upper extremity by the pressure of these large glands on the veins led to the deposition of thrombi. The right upper extremity was very œdematous, and when cut into, serum flowed out freely.—*February, 5, 1870.*

Amyloid Disease of the Liver and Kidneys.—DR. HAYDEN said, the specimen now before the meeting was taken from the body of a man, a cab driver, admitted into the Mater Misericordiæ Hospital, on the 9th of February. The viscera furnished a good example of what was called amyloid degeneration. The history the man gave us was, that he had had syphilis twenty years previously, of which, indeed, there were unmistakable traces visible on his body.

Three months before his death he complained, for the first time, of huskiness of voice, and shortly afterwards he began to cough. When admitted to hospital, his condition was as follows:—He was remarkably weak; his pulse was quick, but regular; his voice was almost entirely gone—he could scarcely make himself heard; he had an incessant and worrying cough, with copious purulent expectoration; the abdomen was inflated, and there was a good deal of liquid in the peritoneum; there was slight œdema of the lower extremities, but no hectic symptoms whatever, neither was there any spitting of blood. Such an examination

as the inflated state of the intestines admitted of led to the discovery of a partial enlargement with considerable hardness of the liver. The spleen was found also to be enlarged; the urine was of low specific gravity and loaded with albumen.

The evidence yielded by a physical examination of the chest was entirely negative; little air entered the lungs. The man was exceedingly low, and was attacked a few days after admission by diarrhœa, which, added to the previous weakness, terminated his existence.

On examining the body, the kidneys were found to be in a state of considerable hypertrophy. The two organs weighed, respectively, 10 and 10½ ounces. The cortex was greatly thickened, being in some places nearly half an inch in depth; and treated with iodine, it exhibited the usual evidence of what was called amyloid degeneration, showing a mahogany-brown tint. The organ was more or less transparent, like bees' wax. The cortical structure, when examined under the microscope, was found to have all the appearances of amyloid degeneration; the glomeruli were enlarged and congested, and received a deep brown tint from iodine. The spleen was found enlarged, but not otherwise altered. The liver was cicatrized on the surface in several situations. It was rough, remarkably firm to the touch; and on microscopic examination, it was found to present an example of a twofold form of disease—namely, cirrhosis, and incipient amyloid degeneration. There was a decided excess of the fibrous structure of the organ, and the cells in most instances were found enlarged, and, together with the lobular vessels, received a deep brown colour from iodine.—*February 12, 1870.*

Tumour of the Uterus.—Dr. ATTHILL exhibited a specimen taken from the body of a woman, who had been under his care in the Adelaide Hospital. Her history was briefly as follows:—She was married, aged 53, the mother of three children, and had been a hard-working and healthy woman. About five years ago, the catamenia ceased to appear; but, after the lapse of a year, a sanguineous discharge returned, and continued to recur with tolerable regularity, till two years ago, when it became more and more profuse, and eight months since became almost continuous. About that time she first observed a large tumour in the abdomen, which, at one point on the left side, was extremely tender to the touch. When admitted into Hospital, she was in an exceedingly anæmic condition, and seemed worn down by excessive loss of blood. She also presented that cachetic appearance which we are in the habit of connecting with malignant disease. In passing the hand over the abdomen, he could easily detect a large tumour rising out of the pelvis, and extending as high as the umbilicus. It lay a little to the left side, and at one point on that side, and there alone, she complained of pain. It was movable; and on making a vaginal examination, it was found to consist

of the uterus enlarged, but movable; the uterine sound passing into its cavity to the depth of at least six inches. Clearly, the tumour was uterine, the only question being whether it was due to hypertrophy of the entire organ, or to a polypus contained within its cavity. To clear up this point, he dilated the os by the introduction of sea-tangle bougies. This was accomplished without much difficulty, and at the end of ten hours a large tumour was easily felt projecting through the os. As the patient's condition rendered it absolutely necessary that this should be removed at once, he endeavoured to do so by means of Dr. Marion Sims's intra-uterine ecraseur. He could feel the lower portion of the pedicle of the tumour, but the upper edge of it was beyond his reach. Having failed with Dr. Sims's instrument, he then used the ordinary wire ecraseur, and succeeded in snaring the pedicle with it. Three times he succeeded in passing a wire round the tumour; but on each occasion it broke. As, however, the pedicle had been subjected to great constriction, he hoped its vitality might have been destroyed, and that it might come away by sloughing. After the patient had been removed to bed, she suffered a good deal from sickness of the stomach, but which, however, under the use of opium, subsided. Matters went on well for three days; indeed, on the third day she expressed herself as being perfectly well. She had no pain on pressure; the pulse was quiet, and the irritation of the stomach had subsided. There was, however, a very fœtid discharge from the vagina. On the night of the fourth day she was suddenly seized with a violent rigor, complained of intense pain over the abdomen, sank into a state of low muttering delirium, and finally died comatose.

On opening the abdomen hardly any trace of peritoneal inflammation presented itself; but on raising the omentum, that spot on the fundus of the uterus which, as previously noticed, had been always excessively tender to the touch, was seen to be perfectly black, and in a condition of actual sphacelus.

On laying open the uterus itself, it was found to contain the enormous polypus which he now exhibited. Dr. Atthill then demonstrated the various parts of the specimen, and called attention to the circumstance that adhesion had, since the operation, been formed between the uterus and the surface of the tumour, which exhibited the marks where the wire of the ecraseur had cut into it.

Glycosuric Amblyopia.—DR. FITZGERALD said, on the 3rd of December last, at the request of Dr. W. Moore, I visited Sir Patrick Dun's Hospital, for the purpose of examining the eyes of a patient admitted under his care for diabetes. The case is briefly as follows:—Thomas Low, aged forty-two, a blacksmith, admitted into hospital last November, stated he had always enjoyed good health until last March, when he first began to suffer from the disease for which he subsequently sought admission into

Fig. 1.

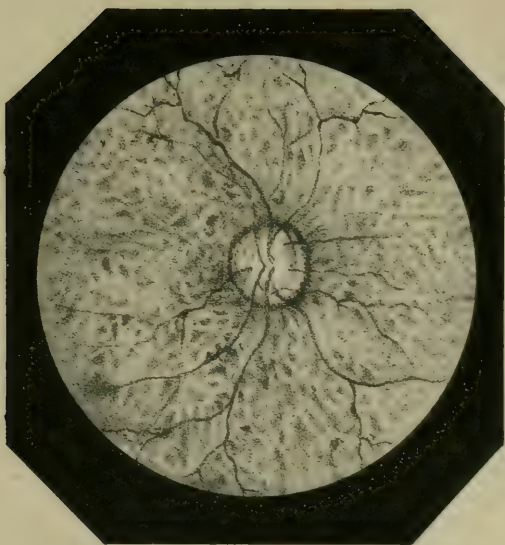


Fig. 2.



hospital. He described himself as having lost flesh considerably, and as suffering from great weakness. At first he said he experienced intense thirst, but since he had been under treatment it had greatly diminished. At this time he was passing large quantities of urine, on an average from six to eight quarts daily. It had a mean specific gravity of 1037. A portion was submitted to Professor Jellett for examination with the saccharimeter. He found it contained 36 grs. of sugar to the ounce, so that the amount of sugar excreted daily was upwards of a pound.

The patient stated that his sight had always been very good until about a month ago, when he first noticed it becoming dim. Some years before he had received an injury, which had destroyed the sight of the right eye; he had now barely the perception of light in it. For the last two years he had been in the habit of using a seven-inch convex glass for reading with the left eye; the use of so strong a glass as this seemed to imply a far higher degree of presbyopia than is met with in individuals of this patient's age. I find, however, that Trousseau speaks of "premature presbyopia" (*presbytie prématurée*), as one of the most frequent and remarkable symptoms in diabetes.^a I am not aware of this having been noticed by any other author.

The evening before my visit, Mr. Harvey, resident medical scholar, instilled into each eye a solution of atropine. I found the right pupil only slightly dilated, but the left pretty fairly so.

Left eye.—Oblique and direct illumination showed that the media were perfectly clear. The ophthalmoscopic examination showed that there was a deposit of pigment at the upper and outer portion (inverted image) of the optic disc, the circumference of which was well marked, being also pigmented; the disc was somewhat paler than normal. On the whole, I am inclined to think there was some amount of atrophy of the nerve. The vessels appeared healthy, and I failed to discover any hemorrhagic spots, generally so characteristic of this disease. There was one very striking appearance, however, which is rather difficult to represent in a drawing—namely, the extraordinary distinctness with which the choroidal vessels and pigment were seen; this was not specially confined to one portion, but seemed to be pretty general throughout the whole of the fundus.

Of the pathological conditions in this case I am not in a position to speak at all positively. The presence of cataract, in cases of diabetes, was a fact long ago recognized; but I think it is only within the last few years that attention has been directed to cases in which there was impairment of vision without any opacity of the lens; the anatomical changes in these cases were, I think, first pointed out by M. Lécorché, who found them to consist of atrophy of the retina, frequent cupping of the optic disc, fatty granulations in the interstices of the nerve fibres, and

^a Trousseau Clinique Medicale, Vol. ii., p. 745.

diminution of the calibre of the vessels.^b M. Testelin has reported a curious case of glycosuric amblyopia following an injury in which no hemorrhagic spots were present; the left optic papilla appeared pale and the vessels small.^c My friend, Dr. Galezowski, in a report read at the Ophthalmological Congress in Paris,^d mentions a very interesting case, in which not only hemorrhagic, but also fatty spots resembling those met with in albuminuric retinitis, were present; an inability to distinguish different colours was also observed in this case; I examined the patient in the present case, but found his appreciation of colours perfect. In the *Dublin Medical Press* of 1862, Dr. William Moore has published a case of diabetes mellitus; the patient's eyes were examined by Dr. Hildige, who found the left optic nerve marked at its circumference here and there with spots of apoplectic exudation; faint greyish spots were observed scattered over the fundus of the right eye.^e I am inclined to look upon the case I have brought forward as one of atrophy of the retina—a condition admirably described by Soelberg Wells in one of the latest and best works on ophthalmology. "It may," he says, "be partial and confined to certain portions or elements of the retina, or complete, the whole retina becoming greatly attenuated, and changed into a thin, transparent fibrillar connective tissue, which is so delicate that the details of the choroid can be seen with unusual distinctness."^f Of course, in such cases, the epithelial layer of the choroid must be implicated, but this is only natural when we consider its very close relation with the external layers of the retina. Ritter, indeed, looks upon it as part of the retina.^g

The conclusion I have arrived at is of an hypothetical nature, and, I am well aware, open to some grave objections, still, in absence of the more positive signs usually present in these cases, and bearing in mind the existence of atrophy of the retina in such cases as pointed out by M. Lécorché, I venture to look upon it as not wholly illegitimate.

Right eye.—Of the right eye the patient gave the following account. Seven years ago, he received a blow of two iron sparks in the eye, which was followed by keratitis; the whole cornea became obscured, but regained its transparency in about three or four weeks, the patient being under the care of Dr. Brown, of Belfast; after this he was able, for a time, to distinguish objects a little, but gradually he lost the sight entirely; at this time he had but a very slight perception of light; occasionally he suffered from attacks of ciliary neuralgia. The cornea presented two slight nebulæ, where the sparks struck—one near the centre, the other at the

^b Gazette Hebdomadaire, 1861, p. 720.

^c Annales d'Oculistique, t. xlix., p. 263.

^d Annales d'Oculistique, t. xlix., Mars et Avril, p. 93.

^e Dublin Medical Press, March, 1862, p. 279.

^f Wells, "A Treatise on the Diseases of the Eye," p. 374.

^g Wecker, "Traite Pratique des Maladies des Yeux," t. ii., p. 32.

upper and inner border; a greyish crescentic-like band appeared across the centre of the pupil, which, at first, I thought was merely an opacity of the lens: on close examination, however, I found the iris was quite tremulous, which led me at once to suspect there was a dislocation of the lens. Subsequent investigation confirmed me in the opinion, that the lens, still inclosed in its capsule, had been dislocated downward, and slightly forwards, the crescentic-like opacity being formed by the detached zonule. I found it impossible to get a view of the fundus, the vitreous humour being I suspect highly disorganized.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY,
THIRTY-FIRST ANNUAL SESSION.

DR. JOHNSTON, President.

Injection of Solution of Perchloride of Iron in Post Partum Hemorrhage.
By WM. ROE, M.D., F.R.C.S.I., Assistant Master, Coombe Lying-in Hospital.

MR. PRESIDENT,—I wish briefly to bring before the Obstetrical Society the treatment of post partum hemorrhage by “Injection of a solution of perchloride of iron into the cavity of the uterus.” I do not, however, intend to enter into the literature of the subject (which is as yet rather limited), by detailing the various arguments which have been brought forward both in favour of and against this practice. This will be found ably discussed in Dr. Barns’ “Lectures on Obstetric Operations.”

The importance of the subject is, I trust, sufficient apology for occupying the time of this Society; for every obstetric practitioner knows that there is no more anxious time than that of attending a case where we even dread post partum hemorrhage, not to speak of the reality of its occurrence. I will now lay before the Society the short notes of the three cases in which I have adopted this practice, and where I believe it has been the means of saving human life. It was not tried until other means had been found insufficient, and the lives of the patients appeared to be standing in the balance, and where, I believe, the loss of another ounce of blood would have been sufficient to bring down the beam.

CASE I.—Anne Coleman, aged thirty-five, fourth pregnancy, was delivered on the 18th January, 1870. She had a good labour, the first stage lasting four hours, and the second stage two hours. After the birth of the child, profuse hemorrhage set in. The pupil in charge of the case sent immediately to the Hospital where I happened to be, and lost no time in seeing her. She was then pulseless, and the hemorrhage still going on. Stimulants were at once administered, and the placenta, which was morbidly adherent, was removed; the hemorrhage still went on, the

usual remedies, cold water, &c., not having any effect in controlling it. I then, seeing it a case of life or death, determined to try the perchloride of iron (which I had never before seen used), and having at hand a concentrated solution of the salt in glycerine, I diluted it with about four parts of water, and injected about half a pint of the fluid into the cavity of the uterus, and was gratified to find that the hemorrhage immediately ceased. Not another drop of blood was lost. Although the contraction was not all that could be desired, because the uterus kept alternately contracting and relaxing, the vessels, however, were sealed, for she lost no more blood. She remained in a very doubtful condition for some time, the pulse being scarcely perceptible for six hours after the removal of the placenta, beef-tea and stimulants being freely administered at short intervals.

She was now seen by Drs. Kidd and M'Donnell, with a view of trying transfusion; but as there was some slight evidence of her rallying, they thought it better to wait, at all events.

I will not tire you with the daily notes; but in eight hours after the removal of the placenta she commenced steadily to improve, and has since made a good recovery, being now quite convalescent.

I may mention that, in March, 1867, this patient was the subject of a similar hemorrhage, from which she made a very slow recovery, but nothing unusual occurred during her pregnancy.

CASE II.—Mary Walshe, aged thirty-three, was taken ill in her tenth confinement on the 21st March, 1870, and was delivered of twins, both males, at the full term, after a labour of about four hours, half an hour elapsing between the births. The first was a breech, the second a head presentation. Immediately after the birth of the second child, profuse hemorrhage set in, which quickly reduced the patient's strength. Stimulants and ergot were administered, but the hemorrhage continued, and the uterus showed no disposition to contract (the usual means being had recourse to). The placenta was now removed together with a quantity of coagula, and a solution of perchloride of iron injected into the cavity of the uterus. The hemorrhage immediately ceased, although the uterine contraction was by no means firm or persistent. This was, however, followed by a most alarming degree of collapse, from which recovery seemed all but hopeless. Beef-tea and stimulants were freely administered, and in a short time her condition began to improve, and she obtained some sleep. On the two following days there was some slight abdominal tenderness, which was relieved by turpentine stupes. In about a week afterwards she complained of pain, soreness, and slight swelling of the right leg, which, however, subsided under the use of hot fomentations, &c., and she has since made a good recovery.

CASE III.—Anne Ivers, aged thirty, in her eighth pregnancy, 62, Lower Clanbrassil-street. The pupils were called to see this patient at two o'clock on the morning of the 2nd of May; she was in charge of a nurse, and had been delivered of a still-born child before their arrival. The placenta was retained, and alarming hemorrhage going on. I was sent for, and on my arrival found the flooding profuse, the patient pulseless and cold, the temperature being only $94\frac{2}{3}$. I ordered some brandy while the solution of the perchloride of iron was preparing. The placenta was morbidly adherent, and on its removal I injected a solution of $\frac{1}{2}$ oz. of perchloride of iron in one pint of water into the cavity of the uterus, in the usual way, which appeared to check the bleeding for some time. However, as the hemorrhage returned, and the patient appeared to be all but gone, I determined to try a stronger solution, and accordingly injected half a pint of a solution double the strength of that I had previously used. It acted like magic. There was no more bleeding. The uterus contracted firmly, the contraction being permanent.

As soon as I felt sure I had a genuine contraction, I had her well bound, applying a small compress over the fundus and ordered her 15 grs. of ergot every two hours, with half an ounce of brandy, and beef-tea *ad libitum*.

She progressed favourably, the temperature rising one hour after the removal of the placenta to $96\frac{1}{2}$.

She took plenty of beef-tea and milk for some days, and is now convalescent.

The following is a Daily Register of the Pulse and Temperature:—

	Pulse.	Temperature.
May 2, 16 hours after operation,	135	$103\frac{2}{3}$
„ 3,	129	104
„ 4,	130	$104\frac{2}{3}$
„ 5,	132	$104\frac{1}{3}$
„ 6,	120	103
„ 7,	115	$103\frac{1}{3}$
„ 8,	100	103
„ 9,	109	$102\frac{2}{3}$
„ 10,	104	$101\frac{2}{3}$
„ 11,	100	$101\frac{2}{3}$
„ 12,	100	101
„ 13,	98	100

Thus it will be seen that, upon the third day, we had the temperature at its highest point, being $104\frac{2}{3}$, while the pulse was only 130; whereas, on the fourth day, the temperature had fallen to $104\frac{1}{3}$, while the pulse had risen to 132. However, we find from this date the pulse and temperature both came down; and on the 12th day we have the pulse only 98 and the temperature 100.

I have endeavoured to give, simply and accurately, the facts of the cases as they occurred. I may, however, mention that I used in the first two cases, a concentrated solution of perchloride of iron in glycerine, diluted with four parts of water, simply because I had it at hand, and there being no time to lose; but I have since thought the glycerine acted beneficially, by its antiseptic properties upon the decomposing coagulæ, as there was very little fetor in the lochial discharges which followed.

In the third case I used the perchloride of iron diluted with water only, and the fetor was much more remarkable.—*Read 14th May, 1870.*

TRANSACTIONS OF THE CORK PATHOLOGICAL AND MEDICO-CHIRURGICAL SOCIETY.*

Case of Resection for Disunited Fracture, treated with Carbolic Acid. By WILLIAM COLLIS, S.A.S. Read November 10, 1869.

RAMSGAH SYCE, aged twenty-two, belonging to F. company, R.H.A., sustained a compound comminuted fracture of lower portion of middle third of right tibia, by means of a kick from a horse which he was grooming.

He was taken to hospital, when the injury presented the following appearance:—A contused wound extended an inch longitudinally along the bone, and about one and a half inches across it; the upper portion of the bone was riding to the extent of two inches over the lower, being also impacted into it to a considerable extent, whilst its spiculated extremity was forcibly pressed outwards, extending the sound skin. On passing the finger into the wound three fragments of bone about one inch in size could be felt. The fibula was also obliquely fractured, but not comminuted; little or no bleeding took place, and the patient suffered but slightly from the shock.

The fragments being quite detached were removed; a slight enlargement of the wound being found requisite. The patient was placed under chloroform, extension and counter-extension used, when, after considerable difficulty, the fragments were placed in good position; the wound was then dressed with carbolic acid (one part of the acid to sixty of sweet oil) and hermetically closed—several layers of lint saturated in the lotion, oiled silk, and sheet lead being used for that purpose. The ordinary tibial splints having been applied, and a bandage used, the thigh was flexed on the abdomen, and the leg on the thigh. A mild laxative with an anodyne was then given.

When seen the next morning the patient was found to have passed a good night, no constitutional disturbance having taken place.

* These Reports are furnished by the Secretary, Dr. Purcell.

The patient went on favourably, not a bad symptom appearing.

On the tenth day the dressings were removed for the first time, when the wound was found to have entirely healed up, no suppuration whatever having taken place; the injury was thus converted into a simple comminuted fracture.

Having left Benares for six months' sick leave to the hills at this stage, I personally am not aware of the case during these months; however, a few days after my return, he was again brought under my notice. I now found that the man whom I had left in excellent condition was in a very debilitated state of health in consequence of mental anxiety and extreme pain at the seat of fracture. Upon examination the fracture presented the following appearances:—The upper fragment of tibia overlapped the lower to the extent of about one and a quarter inches, and pressed strongly on the integuments: complete union had not taken place between the fragments, motion being readily obtained by fixing the upper part of leg and moving the lower. With regard to the exact state of the fibula some doubts existed, but it seemed almost certain that union had taken place. No deposit of callus surrounding the seat of fracture could be determined.

The man being again admitted into hospital was given good nourishing food, rum being given as a stimulant. Under this treatment his general health very much improved, but all action had evidently ceased at the seat of injury, and the limb remained not only perfectly useless but a source of intense pain, and an incumbrance to the patient.

The question of either amputation, resection, or some other procedure had now to be thought of. I did not find much difficulty in deciding against amputation for the following reasons:—Firstly—From the man's position in life, he would be left destitute and unable to get his living; Secondly—Knowing the constitution of the patient it seemed likely that other and milder measures would obviate the adoption of this ulterior one, and at the same time secure to the man a good and useful limb; besides, as is common with natives, he objected most strongly to have amputation performed.

With reference to the other measures usually adopted to the treatment of disunited fractures, viz., setons, rubbing the ends of the bones together, ivory pegs, &c., &c., the very unfavourable position of the fragments rendered them inapplicable to the present case. Resection of the ends of the bones, being thus the only course left open for adoption, was resolved on, and before operating I consulted with Surgeon Perkins, 5th N.I., Staff Assistant-Surgeon Jay, Assistant-Surgeon Ambrose, 58th Regt., and Assistant-Surgeon May, B.M.S. These gentlemen entirely concurred in the opinion I had formed.

The patient having been placed under the influence of chloroform I

made a longitudinal incision over the seat of fracture, along the spine of the tibia, to the extent of about four inches; I also made a cut at right angles to this, directly over the protuberance caused by the riding of the fragments; this cut was about two and a half inches in length. The flaps having been dissected back, the bone was exposed and presented the following appearances:—

No surrounding callus existed, although there was some filling up of the interstices caused by the inequalities of the fragments; between the ends of these a considerable amount of callus had been thrown out; this was hard and very firm, but allowed an amount of semi-rotatory motion between the ends of the bones; of these the upper overlapped the lower to the extent of one and three quarter inches. The extremities of the fragments had become smooth and rounded, and all reparatory action had evidently ceased. The oblique fracture of the fibula was found to be completely united, though an amount of longitudinal displacement existed; the shortening from this cause was, however, not considerable; no callus existed about the seat of this fracture.

The necessary operation was simple; I removed the overriding ends of the tibial fragments with the included callus to the extent of about three-fourths of an inch. The space thus formed between the ends of the fragments corresponded to the amount of shortening which had taken place in the fibula. The patient lost but very little blood, neither the anterior or posterior tibial vessels having been met with. Ligatures had to be applied to four small vessels, though simple torsion was sufficient for some superficial ones. The wound was allowed to remain for an hour exposed to the air, as some oozing had taken place; at the end of this time the bones having been placed in good position, and a few sutures introduced to keep the lips of the wound together; the wound was thoroughly saturated with carbolic acid lotion of the strength formerly used; pledgets of lint, saturated with the lotion, were placed on the wound, these again with gutta percha tissue, the whole being made air tight by means of oil silk cloth. The ordinary leg splints having been applied, and a flannel bandage having been wrapped round the limb, the whole was kept in position by a bandage applied from the foot to the knee-joint. The thigh was now flexed on the abdomen, and the leg on the thigh. Thirty drops of tincture opii were now given. This quieted him considerably, but did not induce sleep. About 10 o'clock p.m., (fourteen hours after the operation) a similar dose was given which had the desired effect, and the patient enjoyed a good night's rest.

December 14th.—Patient very comfortable, complains of but little pain; pulse 104; temperature in axilla 100; tongue slightly whitish; skin moist. Ordered beef-tea; dhoe (a kind of Indian pulse). A dose of oil was also given.

From this date up to the 21st of December, nine days after the

operation, the condition of the patient continued most satisfactory, the pulse having fallen from 104 to 92, and the temperature of the body from 100° to 99°. The appetite continued good, and the bowels were opened daily. On this date he complained of some uneasiness about the wound. The dressings having been removed the longitudinal incision was found completely healed, with exception of where the sutures interfered. The transverse cut was also nearly healed up, while the space between the ends of the bones was filled up with plastic material. The ligatures being loose were removed. A very slight trace of pus was found present, resulting evidently from irritation caused by the sutures. The wound was dressed in a similar manner as after the operation.

December 30th.—On the dressing been removed for the second time, the wound was found to be completely healed up, with exception of a small space about the size of a pin, where a suture had been. Callus was forming between the ends of the bones most favourably. The general health of the patient is excellent. Wound dressed as before.

I left Benares about this time, and did not see the case after, but on my arrival in England I heard from the apothecary of the battery that the patient had all but recovered, and could walk about with the aid of a stick. I have no doubt now that the man is at his duty, and earning his living.

Tetanus in an Infant Nine Months Old, in Connexion with the Round Worm (Ascaris lumbricoides) and Teething. By A. LEITH ADAMS, M.B., Surgeon-Major, $\frac{1}{2}$ Regt.—A case similar in many respects to the following is recorded in the *Medical Times and Gazette*, March 27, 1869, by Dr. Whitehead; with these differences however—whilst in both instances there were no doubts as to the genuine tetanic character of the spasms (as compared with the occasional anomalous convulsive actions sometimes observed during dentition). Dr. Whitehead's case occurred in a younger child without any apparent indications of either dentition or the presence of parasites in the intestinal tract or elsewhere. I take it, therefore, that the following case is interesting with reference to the age of the patient affected, and the presence of round worms, and the irritation of teething, more especially with reference to the probable connexion between one or other or both of the two last named conditions, in bringing on the tetanic convulsions:—

An infant nine months old, reported never to have ailed up to the 29th of March, 1869, when the mother noticed slight twitching of the cheeks, restlessness, heat of head, and constant tendency to carry the hands towards the mouth. I saw her three days afterwards, and in addition to these symptoms, found the pupils much dilated and sluggish in their movements. An indescribable vacancy of expression characterized the countenance.—Pulse, quick; breathing, rapid.—Appetite

good however, but stools had been dark coloured and very offensive for several days. The gums much inflamed; two upper canines just appearing, and the four incisors through; the back teeth evidently coming up rapidly. The mother was a very delicate woman, with a wretched milk-supply, on which the child had been altogether supported, and, excepting water, had (on the parents' authority) never swallowed other nourishment. The gums were freely lanced, and an aperient containing rhubarb with chalk and mercury given; cold was applied to the scalp. Next day the bowels discharged a quantity of dark and very offensive stools. I especially remarked the twitchings of the muscles of the face, that when present the pupils invariably dilated and then contracted in the intervals, but not to the normal extent; the face became congested during the spasms, which, up to this time, were confined to the head. During the next twenty-four hours the limbs participated in the convulsive actions, during which the patient moaned and screamed alternately, and was very restless during the spasms, which were frequent. Still appetite good, and no difficulty in swallowing.

Next day I found her very much worse; there was much flatulent enlargement of the abdomen, much tossing about, and constant spasmodic actions; slight trismus, but swallowed food when placed on the back part of the tongue. Towards afternoon I could clearly perceive the bending backwards of the body, when the jaws became firmly fixed, and there was perfect inability to swallow. In the early part of this day a round worm came away with a stool, which looked more natural than before, but still very mucous. The worm (a female of *A. lumbricoides*) was $3\frac{1}{2}$ inches in length. Santonine in castor oil was forced down, and a free evacuation followed, but the symptoms were rapidly increasing in severity. The trismus, opisthotonos and other tetanic symptoms continued to increase, and on the following day she fell into a semi-comatose state and died—the duration of the illness lasting from 8 to 9 days. Chloroform, opium, and the usual routine treatment were pursued as the symptoms warranted, but, I need scarcely remark, with the same discouraging results that characterize the therapeutic treatment of tetanus.

Remarks.—No *post mortem* examination was allowed, therefore, as regards the parasite we are left in ignorance whether or not more existed in the alimentary passages—but even supposing such was the case, it comes to be a question how far they had anything to say to the tetanus. Again, the irritation of the teething is said to induce convulsions, because convulsions occur during dentition. I dare say there are here more than one practitioner who has frequently seen children affected with convulsions at other times—bad food, excessive eating and drinking, readily induce epileptic and other conditions in connexion with the cerebro-spinal axis—perhaps an ænemic state of the brain from poor nourishment might call forth the states, whether epileptic or tetanic, or

whatever we choose to designate those terrible convulsions, of the true pathology of which, it must be confessed, we are profoundly ignorant. It was strange that long before the tetanic trismus appeared, when there were only mere twitchings observed, that the grandmother of the child should have given it as her opinion to me that the child was suffering from worms, and this leads me in conclusion to state, that this species of round worm is exceedingly common in New Brunswick, where the above case occurred—indeed there are few of the poorer, and even the better classes in the inland towns, that have not been infested in youth or afterwards by the parasite. But the curious part, and one that seems to me tending towards a positive proof that one of the chief ways these worms are introduced into the body is by well-water, is the fact that they occur in infants who never swallowed other than their mother's milk and spring water; and when we think of the sanitary conditions of towns and villages in many parts of the Old and New World, and the infinitesimal numbers of the ova contained even in the segment of a tape-worm, how tenacious is the vitality of the germ and other well ascertained data in connexion with the transmission of animal parasites from without into the bodies of man and other animals, we must believe that there is no more likely vehicle than well-water for the transference of the ovum of the round worms. It comes, therefore, to be a sanitary question for the officer of public health, how far the marked prevalence of one or other forms of worms in a community is owing to defects within his power of remedying. It is surprising (as many have observed) that persons may be absolutely infested by tape, round, or thread worms without apparently suffering in health, being often unconscious of their presence until discovered accidentally. The same apparent innocuous condition is observed in the lower animals; but on the other hand, bad alimentary nutrition oftener accompanies all forms of intestinal worms, and symptoms both subjective and objective, are sufficiently pronounced in by far the majority of instances. I have always noticed, that especially with the round worms, that their presence was simultaneous with large quantities of intestinal mucous, on which they possibly feed. I have had many cases under treatment, and will say that of all remedies santonine is the best for the large round worm, and when given with castor oil, there is never apparently that unpleasant physiological effect on sight so frequently the case when given in frequent small doses. I have followed Kuchenmeister's plan, and give the santonine on butter and bread at night, followed by castor oil in the morning; in this way I lately (with three doses) removed no less than eleven specimens of large round worm from a boy seven years of age. As to the (oxyurides) or thread worms, perhaps of all the three species *the most difficult* to eradicate from children, and often adults, I should like to know regarding the treatment, what remedy or remedies have proved successful in the hands of any of the

gentlemen present. As to iron, I have given it in doses until the patients' stools were thoroughly impregnated with the metal for weeks, and after all found the worms not only alive but flourishing. The salt water enema at bed-time makes the lower part of the intestinal tract unpleasant to them, but that does not cure. I have known instances of some ten to twelve round worms having crawled into the gullet from whence they were withdrawn; and stories were told me by settlers in New Brunswick, of children having wasted away and died—all the time constantly passing large round worms by the mouth and rectum.

The vulgar idea that apples and fruits contain the germs of thread worms, and are the usual means by which the parasite is introduced into the alimentary canal, has not been proven, although quite possible, and even as likely as any other vegetable product eaten in a raw state. We know that beef, pork, &c., produce tape worms; and, no doubt, the habit of eating raw bacon and meat, so common among navvies and also many savage tribes, are the vehicles by which tape worms and other entozoa are introduced.

If it could be shown wherever the round and thread worms abound, that the inhabitants drink well-water subject to surface or other pollution, or are much addicted to the use of raw vegetables, with as much certainty as we can promise the eater of measled pork that he will be infested by the tape worm, then I say it will be the duty of officers of health to insist on proper filtration of water for drinking purposes, and the village well, so long supposed to be the most necessary and healthful institution in the district, must be either shut up altogether or placed beyond the possibility of contamination; whilst water companies will be compelled to furnish a well-filtered pure water.

I have brought this case before the Society and offered these few remarks on human parasites, mostly with the view of eliciting information from the Society, in connexion with the prevalence or otherwise, in Cork and its neighbourhood, of any one of the species just mentioned. I shall, therefore, be very glad to hear any remarks from Members on this subject.—*November 24th, 1869.*

Case of Cancerous Disease of Stomach under DR. WM. TOWNSEND. Reported by JOSEPH O'BRIEN, M.A., Clinical-Assist.—Michl. M^cCarthy, a labourer, aged forty, was admitted into hospital on the 30th October. Owing to his imperfect knowledge of English and considerable natural stupidity, it was found impossible to obtain a connected history of his case. It appears, however, that he was of extremely intemperate habits, and that he had been out of health for some months previous to his admission.

At the time of his admission into hospital he complained of diarrhea, pain in the epigastrium, and great debility. He was treated for these

symptoms with five-grain doses of Dover's powder every 4 hours, and 6 ounces of port wine, with milk and beef-tea, daily. The diarrhea was soon checked, but there was no visible improvement in other respects.

The diagnosis was soon pronounced that he was suffering from malignant disease of the stomach. His appearance was remarkably typical of the cancerous cachexy. The skin was of a dirty yellow colour, the features pinched and worn, and the body greatly emaciated. The powers of life were all impaired. His strength was completely prostrated, his mind despondent, and his appetite bad. Cold sweats occasionally poured from the skin, and he fainted some few times. He got no relief from the constant gnawing pain which he felt deep-seated in the left hypochondrium; and sometimes severe pain was felt in the precordial region, extending upwards beneath the sternum. His tongue was peculiar. It was hard, rough, dry, and raw-looking, seeming to be almost entirely devoid of epithelium. He had no cough, and there was no sign of disease in either lung, with the exception of slight dulness on percussion in the left mammary region. The sounds of the heart were normal, and the pulse beats about 80 per minute.

Opium was prescribed in doses of 1 grain every fourth or sixth hour, to relieve the pain; and he was supported with milk, arrow root, beef-tea, and 3 ounces of brandy, daily.

His history for the month he was in hospital was simply that of increasing emaciation, with gradual loss of strength, and constant pain. He never vomited; but the appetite was extremely delicate throughout. His intellect, such as it was, remained perfect to the last.

Post mortem examination revealed large scirrhus deposit at the cardiac end of the stomach, and in the spleen. The stomach small, and its coats thickened and corrugated. There appears to have been extensive ulceration at the cardiac end communicating with the spleen. The hilus of the left kidney is also much diseased, and the organ itself much enlarged. All the parts in the left hypochondrium were firmly united by adhesions.

The pericardium contained about 6 ounces of serum, and was adherent to the auricles, but not to the ventricles of the heart.

The lower portion of upper lobe of left lung and the lower lobe, are extensively occupied by cancerous deposit, of the firm medullary type.

The liver and right lung and kidney appear healthy. The diaphragm was extensively adherent to all the adjacent organs.—South Infirmary, Dec. 8th.

A Case of Contused Wound of the Vulva in a Pregnant Female. Death; Reported by F. A. PURCELL, M.D., Physician to the Cork Fever Hospital, and Medical Officer to the Douglas Dispensary, Hon. Sec. of this Society.

I WAS called on the morning of the 23rd November, 1869, by a pensioner

named Bradley, who said that on the evening previous his wife had died suddenly, without her being able to see her priest, who had hurried to her in all haste; that the priest had requested the police to have an inquest held, as also that I should be summoned to deliver her, she being about eight months with child; that after her death the child was felt stirring within her.

I accordingly went to the deceased's house, and, on consultation with the police who were there in attendance, procuring evidence for the inquest, and with the earnest solicitations from the friends of the deceased to deliver the woman, I proceeded to make an autopsy as to the cause of her sudden death.

The history, as elicited by the evidence of her mother-in-law before the coroner, was as follows:—"That the two women, she and the deceased, were proceeding to bed, at their usual hour, ten o'clock; that the deceased had undressed, and had mounted on a chair which was alongside the bed, in order to get into the bed, when the deceased fell forwards with the chair to the ground, coming down on her left buttock. That when she went to help up the deceased, she cried out 'she was done for,' as a flooding had come on which deluged the floor; on being placed into bed the hemorrhage still continued, and, knowing her time of pregnancy, which was about eight months she ordered her nurse-tender to be summoned (who happened to live within a door or two of the house). That she went into a 'weakness,' from which she slightly rallied and took a few sups of coffee, but went off again, and died in fifteen minutes from the time of the fall; that previous to this she was apparently in perfect health; that she was pregnant with her sixth child; that the husband was sitting beside the fire in the kitchen, and on her being placed in bed, was sent with haste for the priest; that no violence had been done her."

I proceeded to make a *post mortem* examination, fifteen to sixteen hours after death; body in rigor mortis, hands clenched, with the thumbs of both hands held between the third and fourth fingers; features of her face blue and sunken; the earthen floor beside the bed was saturated with blood, and the side of the bed smeared, where she had sat on the edge when raised from off the floor. Her night-dress and bedding under her were saturated with blood; her abdomen high and prominent; blood coagulated in the hair on the pubis; no marks of violence to be found about body or legs; left leg flexed on the thigh, and thigh on the body, being drawn up and stiffened in that position from rigor mortis.

With such rigidity of the legs, I could with difficulty examine her per-vaginam with my finger, and could not reach the os uteri. I entered the uterus by a semilunar incision through the walls of the abdomen, on the left side, and, on the point of the knife entering the uterus, the liquor

amni shot out in a stream ; having let this escape, I examined the uterus with my hand, and found the os uteri internum quite occluded, as I anticipated from the presence of the liquor amni; the placenta was adherent to the right, and somewhat anteriorly. I then withdrew the child and cut the cord and tied it as after delivery ; no escape of blood could have taken place from the uterus. I now cut down, above the pubis, into the vagina ; holding it tense by tension on the uterus, I could find no blood-vessel open anywhere within the vagina. I then turned over the body on to the left side to examine the anus for hemorrhoids ; none were present, and the feces (soft) escaped without any tinge of blood ; on looking anteriorly I observed some shred of integument beside the vagina, to the right side ; this, at first, I overlooked, until failing to find the cause of the fatal hemorrhage elsewhere, I forcibly opened the legs, and, on examining carefully about the vagina, I found, within the right labium majorum, extending longitudinally downwards and backwards, having the meatus urinarius intact on its inner side, a rent an inch and a half long, the edges of which appeared cleanly severed, extending into the cellular tissue ; I could find no mouth of artery gaping, and the wound was devoid of any coagula of blood ; parts flaccid and lying in apposition. Hemorrhage (in my opinion) then took place from here, in the first instance ; her syncope was probably caused by laceration of some artery (branch of the internal pudic), from which she partly rallied, but soon succumbed to the copious venous loss. When or how did this rent occur ? Did she faint when she was standing on the chair, which caused her to fall forwards bringing the chair with her, and that, on her coming to the ground, the violent concussion caused the rent, or was this part partially opened prior to her getting up on the chair, and from loss of blood syncope came on, causing her to fall, and that on falling the rent was enlarged ? If this be the opinion, what caused the division of the parts in the first instance ?

Taylor states, in his *Medical Jurisprudence*, 7th Ed., page 368 :—
 “Contused wounds on the female genital organs sometimes prove fatal by the laceration of the parts leading to great loss of blood. . . . There may be such a loss of blood in these cases as to destroy life, although no large blood vessel is implicated in the injury. . . . A kick on the vulva, or a fall on this part, may produce a rent which may be mistaken for a cut, and, unless carefully examined, may lead to the inference that a weapon had been used for its production.” Mr. Gutteridge communicated to the *Lancet* (Oct. 31, 1846, 478), a case in which a contused wound of the clitoris proved fatal ; the woman received a kick from her husband in the lower part of the abdomen, while she was in a stooping posture ; she died in about three-quarters of an hour afterwards ; she is not reported pregnant ; on inspection there was no injury to the uterus or vagina ; the wound was situated at the edge of the vulva, extending from the pubis

along the ramus of that bone. It was about an inch long, and three-quarters of an inch deep. From this the fatal bleeding had taken place.

A Report of Three Cases of Cerebro-Spinal Fever, or Black Death, with some few Remarks. By F. A. PURCELL, M.D., Physician to the Cork Fever Hospital, and Honorary Secretary to the Cork Medico-Chirurgical and Pathological Society.

In the nomenclature of diseases drawn up by a joint Committee, appointed by the Royal College of Physicians of London, and forwarded to medical men by authority of the Registrar-General, at page 5, under "General Diseases A, No. 8," is described "Cerebro-Spinal Fever." Synonyms—Malignant purpuric fever, epidemic cerebro-spinal meningitis. Definition—A malignant epidemic fever, attended by painful contractions of the muscles of the neck, and retraction of the head. In certain epidemics it is frequently accompanied by a profuse purpuric eruption, and, occasionally, by secondary effusions into certain joints. Lesions of the brain and spinal cord and their membranes are found on dissection.

This represents in description the cases I am bringing forward, taken from a few that were admitted to the Cork Fever Hospital during the past twelve months.

Before giving the cases, I beg to refer you to the "Army Medical Department Report," for the year 1866, Volume viii., under Appendix No. 45, page 426, on "Cerebro-Spinal Meningitis," with a selection of cases by several medical officers.

It states that this is no new disease, but one which has prevailed at different times, both in this country and on the continent. Its re-appearance in North Germany in 1865, in an epidemic form, its prevalence among the recruits of the American armies, and the occurrence of several attacks among our own troops serving in Ireland and elsewhere, have given it fresh interest and importance at the present time.

This disease appears to have manifested itself under one of two forms.

1. The rapid variety, attended with purple blotches, embarrassed respiration and circulation, amounting to imperfect collapse, and followed by a state of sopor and insensibility, which speedily advances to coma.

2. The cerebro-spinal form, in which symptoms referable to irritative disease of those parts are prominent, such as retraction of the head, pain and cramps of the muscles, hyperesthesia of the surface, delirium, &c., accompanied by fever, and frequently with the effects of disordered innervation, such as herpetic eruptions, &c., about the face and body. A sudden seizure with gastro-enteric functional disturbance being common in many instances of both types.

In reference to its varieties in point of gravity and fatality, and in the absence of any, or any well-marked, anatomical lesions of the nervous system in the first form, it must be remembered, that in these respects,

if it be a fever depending upon a specific blood poison, it follows the law of all acute specific diseases, in that the poison may kill quickly, and be attended with no other marks than those referable to a disorganized blood; while, if the patient survive long enough, the anatomical lesions characteristic of the disease become developed, and to those secondary lesions the symptoms during life, as well as the fatal issue of any given case, may be referred.

From the eleven cases of this disease admitted into Cork Fever Hospital during last year eight died, showing its great mortality; and from my observation of it, it has these three well marked varieties:—

1°. The cerebro-spinal form, in which symptoms referable to irritative disease of those parts are prominent, such as retraction of the head, pain and cramp of the muscles, delirium accompanied with fever.

2°. The cerebro-spinal form, accompanied with purple blotches, appearing on the body or legs, where retraction of head and pain are prominent, and, *vice versa*, where slight retraction, the more prominent the rash.

3°. The rapid variety, attended with purple blotches, embarrassed respiration and circulation, amounting to imperfect collapse, and followed by a state of sopor and insensibility, which advances to coma, excessive vomiting of green matter, if protracted by treatment, edges of rash become pustular, drying, and surface peeling off, leaving dark, blackish patches of coagulated blood. Where pressure or irritation occur, the piece sloughs out, leaving a nasty, deep, excavated sore.

CASE I.—John Linehan, aged fourteen, admitted to Hospital 27th February, 1869, from Mr. Pollock's flax-mill, at Douglas; ill 24 hours. 6 o'clock p.m.

Pulse; 96; weak and feeble; tongue foul; bowels unmoved. Complains that he felt ill on yesterday morning; but went to his work as usual. At four o'clock in the afternoon he was obliged to return home, feeling unwell. His stomach became sick and he vomited green, bilious matter. That on this morning (27th), for the first time, were observed peculiar black patches on face, both arms and legs, and on body before and behind. The larger patches were slightly elevated, with a solid resisting feel; the smaller patches level with the surface, each having edges sharply defined. Interspersed between these larger patches is a petechial rash. Eyes suffused; head hot; pulse feeble; respirations hurried; countenance anxious; tongue furred; sordes on teeth; abdomen full, and slightly tympanitic.

To have 2 ozs. of whiskey in punch, statim; head to be shaved; wine 2 oz.; and bath; for the night.

Hyd. c. Cretâ, grs. iii.

Pulv. Doveri, grs. ii., M. ft. pulv. H.SS.

Haustus oleosus cras mane Sumendus.

February 28th, Morning, 2nd day.—Pulse 120; tongue foul, edges red; one solid stool; slept fairly; no delirium; patches remain same, larger ones look gangrenous.

To have a common enema immediately; a teaspoonful of yeast in water every third hour; bath and wine 4 oz.

February 28th, Evening.—Pulse 120, full; eyes suffused; bowels unmoved since morning visit.

Apply three leeches behind each ear.

R Calomel, grs. iii.

Pulv. Jacobi ver. grs. ii., M., H.SS.

Apply pole blister during night, if any brain symptoms threaten.

March 1st, Morning, 3rd day.—Pulse 105; tongue creamy; stools 2, solid and large; leeches took well; eyes less suffused; surface doughy. Complains of a feeling of deadness in his arms.

Ordered a glass of porter in place of 2 oz. wine.

March 1st, Evening.—Pulse 114; found him asleep; rash on face somewhat faded, as also that on arms; still complains of deadness in his arms; presents an improved aspect.

March 2nd, Morning, 4th day.—Pulse 90; tongue cleaning; stools 2, solid; herpetic rash appears on lower lip; the feeling of numbness in arms and legs somewhat better; rash somewhat more faded; red margin still marked.

March 2nd, Evening, 4th day.—Pulse 114; stool nil; slept during day; the red margin of rash extending more outwards, and the centre changing to a yellowish black; petechial rash on face somewhat faded.

To have an alterative powder and purging draught in the morning.

Omit all wine.

March 4th, Morning, 6th day.—Pulse 108; tongue cleaning; stools 2. Complains of the numbness in right arm; that of the left better; right elbow joint swelled; rash on right arm somewhat more inflamed than that on the left; face clearing; a pustular margin is formed around each black spot, and a red areolar, from a quarter to half an inch in depth. This change in character of rash resembles variola.

Mr. Heazle is kindly going to make a drawing of legs and left arm.

March 5th, Morning, 2nd day.—Pulse 105; tongue cleaning; stools 2. Centre portion of patches elevated and containing a darkish fluid; some others scabbing over and drying up; petechial rash fading; right elbow painful, stiff, and swollen.

March 6th, Morning, 8th day.—The black centre portion of patch is faded; pus disappeared, or dried out of pustular margin of patches, the entire scabbing and becoming a cicatrizing sore. Right elbow-joint painful. To have five leeches applied, and afterwards poulticed. Appetite good. To have beef-tea instead of broth, and a small chop.

R Potassii Iodidi, grs. xxiv.

Tinct. Cinchonæ co. $\frac{3}{4}$ i.

Decoct. Cinchonæ $\frac{3}{4}$ vii. M.

An ounce to be taken three times a day.

March 8th, Morning 10th day.—Pulse 108; tongue foul; stools 2; elbow-joint painful; rash, some patches presenting vesicles full of a yellowish serum; some with blackish centre scabbing off; some more, particularly on the elbow, where the poultice is applied, present excavated sores, the centre having sloughed out.

This boy left Hospital on March 27th, several sores still remaining on arms and legs, which are dressed with resin ointment.

This lad was an apprentice to a brass fitter in Mr. Pollock's flax-mill; his father manager over same.

CASE II.—Ellen Clifford, aged twenty, admitted to Hospital at midnight on 29th March, 1869, from Mr. Pollock's flax-mill, at Douglas, where she worked in what is called the preparing room of the mill. Ill 60 hours.

March 30th, Morning, 3rd day.—*History*—On Saturday night, 27th instant, she first began to complain and commenced vomiting; on Sunday night and early on Monday morning a reddish rash was observed on her face by her mother, being more out on her upper and lower extremities than on her body. This rash, during the day of Monday, presented the appearance of blackening from the blueness that first pervaded it. The surface of the skin has a high rosy blush; temperature elevated; hyperæsthesia of surface, particularly over extremities. The extremities are thickly covered with black patches of all shapes, and varying in size, some circular, others like streaks, but comparatively few on the face and body, before and behind, when compared to that on the extremities. There exists on the surface, and between these larger and well-marked patches, a petechial rash, blueish-black in colour.

Pulse 105; tongue dry; sordes on teeth; stools 1; constant delirium and restlessness during night; conjunctiva and cornea greatly congested; what she vomited yesterday, her mother states, was a fluid, dark grass-green in colour.

Ordered four leeches behind each ear.

Calomel, grs. 5, to be given immediately, and to be followed in six hours after by a dose of house medicine.

March 30th, Evening, 3rd day of illness.—Pulse 112; delirium constant; complains of her throat being a little sore; on examination, presents a red, congested, and dry appearance; hyperæsthesia of surface.

Apply a blister to the pole; yeast to be given in water.

March 31st, Morning, 4th day.—Pulse 100; tongue dry; stools 2; spent a restless night; constant delirium; passing under her involuntarily.

The rash has changed somewhat this day, from the blue-black to a yellowish hue.

A glass of porter was taken since yesterday with the yeast.

March 31st, Evening of 4th day.—Pulse 108; tongue, centre dry, edges moist; still delirious; some of the patches are large, centre flesh-purple, circumscribed by a blue-gray, circumscribed again by a crimson-purple, having a rose-tint blending with the adjoining skin.

R Morphiae acet. gr. $\frac{1}{2}$.

Syr. et aquæ ad \bar{z} i. M., H.S.S.

During this afternoon Mr. Heazle took a drawing of buttock and upper portion of right thigh, which I beg to present to you marked.

April 1st, Morning, 5th day.—Pulse 87; tongue moist; stools nil; passing involuntarily under her. The anodyne draft was given at midnight, after taking which she slept off and on till morning, and at this visit is still sleeping; when she awakes she is delirious. The blush of the skin proper has greatly subsided, leaving a rose-coloured margin around the black patches. Eyes still greatly suffused. Porter with the yeast; broth; and to have, in addition, 4 oz. wine; blister to be applied to the vertex of head. Ordered calomel, 2 grs., every third hour.

April 1st, Evening, 5th day.—Pulse 94; stools nil. Ordered a common enema.

April 2nd, Morning, 6th day.—Pulse 110, feeble and compressible; tongue dry; stool 1. Enema given last evening brought away nothing from the bowels. Spent a restless night; respirations 27 in the minute, of a heaving, sighing character; tremor of the hands and arms, synchronous with the heaving respiration; eyes are peculiarly congested, with cornea hazy and opaque—pupils contracted.

Head to be dressed with ung. hyd. et \bar{z} i. of ung. hyd. dil., to be rubbed in every third hour, and to continue calomel powders.

Brandy 2 oz. to be given immediately; wine to be increased to 8 oz.; yeast and porter and broth to be continued.

R Sp. ammonia aro. sp. ætheris chlorici āā \bar{z} i.

Tinct. camphoræ ad \bar{z} vi. M \bar{z} i. every 3rd hour.

Ordered to apply a fly blister to the sternum.

A plain water enema was given early this morning, by order of the Resident Medical Officer, which brought away the one large stool recorded above.

April 2nd, Evening, 6th day.—Pulse 130; tongue moist; stool nil. She is somewhat more conscious at this visit. Ordered a common enema, and her anodyne draught to be given if required.

April 3rd, Morning, 7th day.—Temperature 100; pulse 96; tongue moist, breaking up; stools 2; respiration 20. Spent a restless night. Catamenia have come on. No change in appearance of rash on the lower extremities; one patch under right elbow has a pustular margin around

the black centre, more inside than outside the red circumscribed margin. Slightly salivated.

April 3rd, Evening of 7th day.—Temperature 97 ; pulse 105 ; tongue moist ; stools nil.

To have $\frac{1}{2}$ gr. morphia acet. in draught.

Passes every thing in bed. I am anxious to save her urine. Conjunctiva greatly congested ; cornea opaque. She seems to have no sight in either eye. Pus is observed only around the patch under right elbow, as observed at the morning visit ; but around the others pus is incipiently forming.

April 4th, Morning, 8th day.—Temperature 101·3 ; Pulse 118 ; respiration 30 ; stools nil ; tongue dry. Pustular margin encircles most of the patches on the arms and chest ; the patches on the lower extremity are not in so far advanced a stage. Of these the centre is purple, with about four lines, outside of which is a blue-gray, between which and the outer rose-coloured margin the pustular ring is more than incipiently present.

April 4th, Evening, 8th day.—Temperature 102° ; pulse 120 ; tongue moist ; stools 2. Far more collected. Rash has assumed a change—centre of patch become sunken, edges raised and pustular ; centre at present a greenish-yellow (like a black eye when changing its colour), more particularly that on the arms, face, and chest ; that on the lower extremities much the same appearance.

April 5th, Morning, 9th day.—Temperature 102·3 ; pulse, 106 ; respiration 30 ; stools 2 ; tongue cracked and moist. A tremor in arms and chest, and risus sardonicus slightly present. The patches present a black sunken centre, with an elevated pustular margin, having a tendency to dry and scab over.

April 5th, Evening, 9th day.—Temperature 102·1 ; pulse 120 ; stools purging ; tongue moist. At this visit she is just after a dose of her wine. Eyes suffused, left cornea opaque, with the iris in left eye pulled irregularly upwards and outwards.

Two ear blisters.

R Potassii iodidi, grs. 20.

Tinct. cinchonæ co. $\frac{3}{4}$ i.

Decoct. cinchonæ, $\frac{3}{4}$ vii. M.

$\frac{3}{4}$ i. three times a day.

April 6th, Morning, 10th day. — Temperature 102·1 ; pulse 108 ; stools 3 ; tongue centre dry, edges moist. She spent a restless night, more on account of the purging. Rash drying and scabbing. Omit her brandy.

April 6th, Evening, 10th day. — Temperature 102·1 ; pulse 128 ; tongue moist. Complains of pains in her feet and all over her body. Inability to raise her left arm ; power over the fore-arm, but complains

of pain extending down to the fingers. Rash patches, black centre, with yellow pustular edge.

April 7th, Morning, 11th day.—Temperature 101·8; pulse 116; stools 3; tongue clean and moist. Restless, and slept badly, and is still passing under her, but more collected. Iris of left eye still irregular; left arm, from shoulder to elbow, she is unable to stir, which, if I raise up, falls helplessly down; power over the fore-arm; œdema of both feet, left more than right; patches, centre turning quite black and sunken, and drying at edges.

To have a quart of beef-tea, of three pounds of beef to the quart of water.

April 8th, Morning, 12th day.—Temperature in right axilla, 100·7; in left, 101·2, a difference of 0·5 of a degree; pulse 118; stools 3; tongue moist.

Spent a restless night, in constant delirium, and is at this visit excited and delirious, passing under her in the bed. Paralysis still persistent from shoulder to elbow in left arm; no deviation in the tongue. Œdema the same in the feet, more in left than right.

Apply a blister to vertex of head.

April 8th, Evening, 12th day.—Temperature 102·4; pulse, 123; stools nil; belly tympanitic; tongue dry; surface of skin hot; great thirst.

To have a turpentine and assafœtida enema.

April 9th, Morning, 13th day.—Found sleeping on examination; temperature 102·9; pulse 112; stools 1. Had no sleep during night; constantly screaming from pain in her right axilla, left arm, side, and leg; no delirium.

April 9th, Evening, 13th day.—Temperature 102; pulse 130.

April 10th, Morning, 14th day.—Temperature 101·2; pulse 120; stools 1; tongue cleaning. More collected, and for the first time called for the bed-pan; urine of good colour, acid, and slightly albuminous; pain continuing in left arm and leg, and cries out on having them raised; œdema in feet; some patches on left hip becoming excavated, with pus exuding from some; no sight in eyes, but can distinguish light from darkness; conjunctiva still greatly suffused, but cornea clearing off; left iris still irregular.

April 10th, Evening, 14th day.—Temperature 103; pulse 130.

April 11th, Morning, 15th day.—Temperature, taken somewhat later, 102·2; pulse 114; stools 3. On yesterday and to-day the two larger patches on the right buttock are scabbing over, centre depressed, and pus exuding from its circumference and from beneath the scab; power of left leg, but still inability to move the arm.

April 12th, Morning, 16th day.—Pulse 116; stools 4; tongue dry; sordes on teeth; great thirst; face flushed; febrile symptoms increased;

inability to raise left arm, but when assisted to raise it, a stiffness and partial resistance is given; pain up in the shoulder joint. Rash; slough is cast off from some of the sores, and in some a deep cavity remains with an unhealthy surface.

Chalk-mixture when required to check the purging. Foment the left shoulder.

April 13th, 17th day.—Pulse 111; stools 3; tongue dry. Her left arm somewhat better; ulcers to be filled with powdered bark and camphor, and dressed over with resin ointment.

April 18th, 22nd day.—Pulse 104; stools 2; tongue cleaning.

To have four ounces of wine instead of 8 ozs., as before; omit the iodide of potash and inf. bark mixture, and to have a teaspoonful of the eff. citrate of quinine and iron three times a day.

April 27th, 31st day.—Pulse 102; stool 1; tongue clean.

Spent the night restless and watchful, and again beginning to pass under her in the bed, and only answers on repeatedly questioning her. A general listless appearance, with left cheek constantly resting on the pillow. Her appearance suggests an asthenic condition of the brain, approaching softening of its structure.

Ordered to omit the eff. citrate of quinine and iron. And

℞ Liqr. ferri phosphatis et quiniæ et strychniæ ʒ i.

Aquæ duræ, ʒ vii. M.

ʒ i. ter in die.

To have a desert spoonful of Liebig's essence of meat, given in the quart of extra beef-tea.

4 ozs. claret instead of wine, as ordered on 23rd instant.

May 1st, 35th day.—Pulse 108; stool 1; tongue cleaning; vomited yesterday and this morning; passes all excrements in bed; extremities cold and livid.

To have applied flying sinapism to the chest; brandy ʒ iv. in emulsion, and wine ʒ iv.

May 2nd, 36th day.—Pulse 112; stool nil; tongue cleaning; vomiting continues; aspect greatly improved; heat of surface and circulation good, and spent a good night.

To have an ounce of fluid magnesia when stomach is sick; omit brandy and claret; continue her wine, ʒ iv., and beef-tea, ii.

ʒ iv. brandy at night-time if required.

May 5th, 39th day.—Pulse 110; stools nil; tongue clean; vomiting, especially if pressed with drinks, passing under her in bed; face and neck flurred.

℞ Bismuthi Subnit. grs. 30.

Mucilagin. G. A. ʒ ii.

Aquæ ʒ iv. m.

Two table-spoonsful three times a day.

May 6th, 40th day.—Pulse 120 ; stools nil ; tongue clean ; no vomiting to-day ; has only taken two doses of the bismuth mixture.

May 8th, 42nd day.—Pulse 120 ; stools 1, the result of a common warm water enema ; vomited incessantly since yesterday ; omit wine.

To have $\frac{3}{4}$ iv. brandy in emulsion ; apply a sinapism to epigastrium.

May 9th, 43rd day.—Pulse 114 ; stool 1 ; tongue red and dry ; vomiting everything.

To have per rectum every two hours.

Beef-tea, $\frac{3}{4}$ i ss. Brandy, $\frac{3}{4}$ ss.

To be given lumps of ice to suck.

May 10th, 44th day.—Pulse scarcely perceptible ; tongue moist ; vomiting increasing ; tremor of surface.

To have flying sinapisms applied to chest.

May 14th, 47th day.—Pulse 110 ; stool 1 ; tongue moist ; temperature 100·4 ; vomited once this morning ; stomach remained quiet, until a little brandy and water was given this morning.

Injections of beef-tea and brandy have been constantly given since the 9th inst. as then ordered.

To be allowed to chew a mutton chop, but not to swallow the fibre.

May 15th, 48th day.—Pulse 114 ; temperature 97·9 ; tongue dry ; stools nil.

May 16th, 49th day.—Pulse 120 ; temperature 99·2 ; tongue moist ; stomach quiet.

May 17th, 50th day.—Pulse 138 ; temperature 100·6.

May 18th, 51st day.—Pulse 111 ; temperature 99·4.

May 19th, 52nd day.—Pulse 126 ; temperature 99 ; tongue dry ; stools nil ; eyes slightly suffused ; answers no questions ; passing under her in bed ; presents a decidedly bad aspect, and has become greatly emaciated.

May 23rd, 56th day.—Pulse 159 ; slumbering and unconscious.

May 24th, 57th day.—Died at 1 o'clock a.m.

CASE III.—Batt Killeher, aged sixteen years, admitted to the Cork Fever Hospital under my colleague, Dr. Beamish, about 4½ o'clock p.m. Wednesday afternoon, 2nd June, '69 ; from St. James'-square, Blackpool, and from the employ of Thomas M'Kenzie and Sons, seedsmen. States that he was three days ailing before he took to his bed on yesterday (Tuesday), when he began vomiting greenish matter and purging. On admission, his pulse 120, and scarcely perceptible ; respirations rapid and laboured, with mucous râles over entire chest ; conjunctivæ only "*slightly*" congested (this was particularly noticed as in the previous cases, which this was supposed to resemble, the conjunctivæ were *greatly*

congested). Extremities cold, and several spots resembling purpura maligna, but much larger, one in particular on right thigh, of stellate shape, and about the size of a shilling, level with the surface, and of slate blue colour; both arms, right especially, thickly covered with a rash resembling the mottling of typhus fever; tongue peculiarly “yellow” and furred; limbs powerless; vomiting a yellow matter, which soon turned to a dark brown coffee ground matter.

His head was shaved and a pole-blister applied, and another blister to the sternum; mustard stupes to feet and legs.

Calomel grs. 4. Wine \bar{z} iv. and broth.

Wednesday morning, 3rd June, 10½ o'clock a.m., 6th day of illness.—Pulse 100, and scarcely to be felt; bowels moved twice; breathing greatly oppressed; quantities of mucous churing in the chest; conjunctivæ “greatly” suffused; face livid and swollen; scarcely conscious; skin livid, and patches turned quite black, especially on the lower extremities, and exhibiting more of the characteristic appearance observed in the previous cases; interstitial large petechial rash extensively diffused over entire body; vomiting continues and more frequent.

Death ensued at 11½ o'clock a.m.

Immediately after death the face and body became greatly swollen, and surface of body turned a livid black, resembling much the appearance of the 3rd case, with the addition of the large black patches. No autopsy allowed.—*Read February 9th, 1870.*

A Case of Removal of Left Superior Maxilla. By Dr. HADDEN, Clonakilty.—Mary Kane, aged fifty, the wife of a labourer, was admitted into the Clonakilty Workhouse Hospital, on the 11th February, 1869, for the removal of the left superior maxilla. Three years before a fleshy tumour had begun to grow from inside of the upper gums, and gradually increased in size, without causing any suffering, except from its mechanical inconvenience, until it filled the whole cavity of the mouth, projecting between the upper and lower molar teeth on the right side, and being grooved by them; on the left side it had displaced the superior molars, and connected itself with a similar growth, the size of a hen's egg, springing from the outer surface of the alveolar ridge, and causing the left cheek to project; the anterior wall of the antrum was bulged prominently forward, as high as the infraorbital foramen, while its internal one could be seen forming a rounded protuberance in the left nostril, but not passing the median line; the eye was rendered somewhat prominent, but not otherwise displaced. The upper surface of the tumour in the mouth was so closely adapted to the vault of the hard palate, that the finger could not be insinuated between, but a bent probe passed carefully from the right, proved that the pedicle from which it grew did not pass the median line; when the left index finger was passed round between the

tumour and the cheek at the right side, the growth was found to fill the pharynx, and lie against the bodies of the cervical vertebræ by its inferior extremity, where the finger could be passed between, its nail resting on the spine and its pulp against the tumour, the velum palati and uvula intervening above; by its inferior surface, the tumour lay in contact with the epiglottis and tongue, which latter it caused, by its downward pressure, to bulge into the submaxillary regions when the mouth was closed. The right index finger, when oiled and passed between the left cheek and the tuber maxillare above the smaller growth, reached a hard body imbedded in the back of the tumour, which, with great difficulty could be recognized as the displaced masticating surface of the wisdom tooth; this could also be reached when the left finger was forcibly pressed round from the right side. Just above this point a gum-elastic catheter could be passed round the thick pedicle ($6\frac{1}{2}$ inches in circumference), from which both tumours sprang a notch about $\frac{3}{4}$ of an inch deep, separating them where the molar teeth had been. It was thus evident that a considerable portion of the degenerated maxilla was included in this common pedicle. The left hamular process could not be detected, probably due to the backward displacement of the pterygoid plates of the sphenoid, and the very little room there was for the finger to move. From the examination above described I hoped the disease was limited to the left superior maxilla, and perhaps the palate bone, and determined on removing the former, or both, if necessary.

When she fell asleep for a moment, the most distressing breathing ensued, and she awoke in a few minutes suffocating: indeed, the mystery to everyone was how she breathed or swallowed; yet her general health was good and her spirits cheerful.

Chloroform was plainly inadmissible, though, were it not for the mouth tumour, I would certainly have administered it from what I have seen of its use in the removal of the maxilla, in the practice of my old friend and teacher, Dr. H. H. Boxwell, of the County Wexford Infirmary.

Two difficulties existed in the performance of the operation in this case—First, the usual incisions through the soft palate could not be made; second, when the bone would be isolated, it would be impossible to apply the usual downward pressure to dislodge it, and allow of the division of the remaining soft parts behind. To meet these (having first secured my patient in a stout arm chair, in the usual way), I passed the chain of an ecraseur round the pedicle of the mouth tumour, and also a chain saw, enclosed in a gum-elastic catheter lest it should be required, intending to remove the tumour from the mouth before wrenching the maxilla from its bed. An assistant now commenced to screw up the ecraseur, and the moment the chain tightened on the pedicle, I began my incision through the median line of the upper lip, and continued it round the ala of the nose, to within half an inch of the inner canthus, then dissected the cheek

freely off the tumour, cut through the root of nasal process of the maxilla into the antrum, by one stroke of a powerful bone-forceps, then through its anterior wall, beneath the orbital plate, when I turned the forceps backward and somewhat upward, and severed the bone through its tuberosity, I now extracted the incisor tooth, pushed the lower blade of the forceps between the tumour and hard palate, the upper one lying on the floor of the nose, and with one stroke divided the hard and soft palates. By this time I hoped the *ecraseur* would have been through the pedicle, but finding it was not, and under the impression that my patient was suffocating, I seized the bone in a lion forceps, thrust my left index finger behind the tumour, and with one steady pull the whole mass came away without leaving a shred to be divided. A huge, but perfectly healthy cavity remained, the floor of the orbit looking quite normal, and not a trace of diseased tissue to be found anywhere. The *velum pendulum palati* was intact. One or two small vessels sprang, but no ligature or cautery was required. All oozing having ceased, and the cavity having been filled with pledgets of lint soaked in perchloride of iron, and having numbered ligatures attached, the lips of the incision were brought together by wire sutures, and two hare-lip pins, a single fold of lint wet in cold water being applied over all. To compensate as far as possible for the absence of chloroform, the operation was planned so as to economize to the utmost, time and suffering in the execution of it, consistently with the complete removal of the disease; and thus reduce the danger from shock to the minimum. For this purpose the knife and the *ecraseur* were used simultaneously, the bone-forceps exclusively employed, and the cautery dispensed with; this latter was done with some reluctance, on account of the weight which some eminent surgeons attach to its use, but on the closest examination both by sight and touch, on the part of my kind assistants, Drs. Hadden, Garde, Bennet, Heas, and myself, it was concluded that the parts were so perfectly free of disease, that its use would be a needless infliction of pain on one who had borne her sufferings with astonishing fortitude.

The removal of the disease occupied five minutes, and so little shock was there, that she walked from the chair to her bed, and with a smile expressed her satisfaction at the removal of "the baby she had nursed for three years," (her own words.)

After being settled comfortably in bed she got some brandy and water, with 30 minims of Battley's sedative, she fell asleep in about an hour. The operation was performed at noon, on Tuesday, February 16th. At 11 p.m. her pulse was 92, temperature $101\frac{1}{3}$, the flap warm and but little swollen, the uvula tumid and lying on the dorsum of the tongue; she was ordered beef-tea and milk and water, reaction having been fully established.

Wednesday, 17th.—Pulse 88; temperature $99\frac{1}{3}$; had a good night; spoke

cheerfully and distinctly, no pain, the wound looked well, and the uvula was less swollen.

It would be tedious to follow the case further; not an unpleasant symptom arose; the wound healed completely by the first intention; the pledgets were removed on the third, the needles on the fourth, and the sutures on the fifth day; under the daily use of carbolic acid lotion, the internal parts granulated and healed up kindly; and on the 11th of March she was discharged through the guidance and blessing of God, in the treatment, free, as far as I could judge, from disease.

There are some points of interest about the tumour; those parts of it which projected into the mouth and underneath the left cheek, springing from the inner and outer surface of the alveolar process, seem to be huge epulitic growths; that occupying the antrum is peculiar in being roofed over where it lay against the floor of the orbit by a delicate shell of bone, which appears to be rather a product of the morbid process than part of the maxilla.

The spongy bones being quite healthy, one is led to hope that there may not be a return of the disease.

April 19th.—I have seen this patient to-day (two months after operation), and am thankful to be able to say, that there is not the *slightest* evidence of the return of the disease; attempted to photograph her, but she was so very nervous about it, that I could only get a very inferior negative.

February 21st, 1870.—This patient remains perfectly well, and free from any symptoms of a return of the disease.—*Feb.* 23, 1870.

A New and most Useful Eye-Salve, in "Granular Lids," and all Cases of Chronic Ophthalmia. By JOHN WILLIAMS, Physician and Surgeon.—After long experience I can speak most confidently of this ointment, for the composition of which I now publish the following formula:—

R Arsenici Sulphureti, 2gr.

Unguenti Citrini, 23.

Axungię Preparat, 63.

M. Bene.

In cases of "granular lids," accompanied with most inveterate "pannus," and in almost all cases of chronic ophthalmia, in which the conjunctiva has become almost cuticular, I have found this ointment particularly useful. Ophthalmia is well known to be very prevalent in the city and county of Cork, so that I had very many opportunities of proving the efficacy of this ointment. The upper eye-lids should be everted in cases of "granular lids," and about the size of a hemp seed of this ointment should be applied with a camel-hair pencil, which must be introduced into the superior palpebral sinus, to the diseased conjunctiva. In suggesting this local remedy I am not unmindful of *general* treatment, without which *any* local remedies are almost useless.

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2. Inaugural address delivered on the ninety-seventh anniversary of the Medical Society of London. By John Gay, F.R.C.S., President. London: A. Schulze. 1870. Sewed.
3. On the law which regulates the relative magnitude of the areas of the four orifices of the heart. By Herbert Davies, M.D., F.R.C.P., Senior Physician to the London Hospital. London: Taylor and Francis. 1870. Sewed.
4. A practical treatise on the diseases of children. By J. Forsyth Meigs, M.D., one of the Physicians to the Pennsylvania Hospital, and William Pepper, M.D., one of the Physicians to the Philadelphia Hospital. Fourth edition. Philadelphia: Lindsay and Blakiston. 1870. 8vo, pp. 921.
5. Public health: a popular introduction to sanitary science. By William A. Guy, M.B., Cantab., F.R.S. London: Henry Renshaw. 1870. Fcap. 8vo, pp. 216.
6. Practical notes on the new American remedies. By R. Tuthill Massy, M.D., L.R.C.S.I., &c. London: Edward Gould and Son. 12mo, pp. 74.
7. History of modern anesthetics: a second letter to Dr. Jacob Bigelow. By Sir J. Y. Simpson, Bart. Edinburgh: Edmonston and Douglas. 1870. Sewed.
8. A handbook of phrenology. By C. Donovan. London: Longmans, Green, Reader, and Dyer. 1870. 8vo, pp. 192.
9. Report of the Imperial Academy of Medicine respecting the vaccinations performed in France during the year 1867. Translated and abridged by George S. Gibbs. London: Longmans, Green, Reader, and Dyer. 1870. Sewed.
10. L'électricité appliquée à la thérapeutique chirurgicale, et en particulier au traitement des accidents produits par les inhalations d'éther et de chloroforme. Par M. le Docteur J. Abeille. Paris: J. B. Baillière & Fils. 1870. 8vo, pp. 108.
11. The Medical Institutions of the United Kingdom. By John Chapman, M.D., M.R.C.P. London: John Churchill and Sons. 1870. Pp. 120.
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13. The life and letters of Faraday. By Dr. Bence Jones, Secretary of the Royal Institution. In 2 Vols. Second edition. Revised. London: Longmans, Green and Co. 1870. 8vo, pp. 385 and 491.
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PART I.
ORIGINAL COMMUNICATIONS.

ART. X.—*Remarks on the Operation of Transfusion and the Apparatus for its Performance.* By ROBERT M'DONNELL, M.D., F.R.S., one of the Surgeons to Dr. Steevens's Hospital.

IN the May number of this Journal for the present year Dr. Beatty published a case of *post partum* hemorrhage, in which the operation of transfusion had been performed. I may be allowed cordially to thank him for the flattering terms in which he speaks of the individual who had the good fortune to be the operator in this instance. I may be also allowed to return the compliment by saying that I have seldom read anything at once more truthful or more graphic than his description of the near approach of death and the dawn (as one may almost say) of returning life in the patient. It was one of those cases which repay a surgeon for years of anxiety and toil, and which make up for many disappointments.

The operation of transfusion may be rendered so simple and so safe that I am convinced it should be performed in a much larger number of cases than it is at present. The surgeon should be prepared to perform it not only in all cases of impending death from loss of blood, where it has been possible to arrest the actual hemorrhage, or where it has become spontaneously controlled, but in many other instances. When an operation is simple, almost free

from danger in itself, and as regards its performance nearly painless, it is well to try it in cases in which one would shrink from it were it a dangerous or difficult procedure. Acting upon this principle, I once performed it in a case of tetanus, to which I shall presently refer, and I conceive it is well deserving of more extended trial in such cases, as well as in chlorosis, cholera, and other affections.

At present I wish to confine my observations to two points, one physiological as regards the fibrin of the blood, the second practical touching the apparatus and mode of performing the operation.

I conceive that most physiologists at the present time will agree with me in thinking that it is an advantage to get rid of the fibrin of the blood about to be used for transfusion. Few are now found to believe that the fibrin is an all-important nutritive element (plasma) existing in the blood, without which it ceases to exercise its life-supporting functions. Notions upon this subject have undergone a great change since, some twenty years ago, Mr. Simon drew attention to what is now, I fancy, regarded as the true state of the case. In his admirable "*Lectures on General Pathology*" he says:—

"This is a matter of great importance, and you cannot do justice to it without reflecting carefully on the natural functions of fibrin. Many physiologists have regarded fibrin as that ingredient of the blood which, in the ascending scale of development, stands next for appropriation into the living textures of the body: they have regarded it as representing the ripeness, and perfection, and nutritiveness of the blood. On the opposite side, of late years, have been some who inclined to a very different view, thinking that they find cogent reasons for placing fibrin on the same scale as the extractive matters, and for reckoning it among those elements which have arisen in the blood from its own decay, or have reverted to it from the waste of the tissues. I may confess that, to my mind, this appears infinitely the more plausible view, and I will tell you the arguments which induce me to adopt it.

"First, I find that fibrin is undiminished by bleeding, however frequently repeated: nay, that it often, or even usually, increases under this debilitating treatment; its highest figure given in Andral's book (10·2) was at a fourth bleeding; and Scherer found it as high as 12·7 at the third venesection in a case of pneumonia. I find that under many other circumstances of exhaustion, and weakness, and inanition, during the progress of starvation, during diseases essentially anemic, during violent fatigue, and the like, its

proportion has been found at least as high, perhaps higher, than in the inflammatory process. And as in these respects I find its proceeding to be in direct contrast to that of the red-globules (which we know to be potential elements in the blood, and which are at once reduced by bleeding or starvation), so also do I find a similar contrast in another striking particular. Messrs. Andral and Gavarret, in the course of their extensive researches in the comparative physiology of the blood, ascertained that an improvement in the breed of an animal tended always (*cæteris paribus*) to increase the proportion of its coloured blood-corpuscles; they found that the same improvement tended likewise to diminish the proportion of its fibrin. And I find further indications of the same inverse ratio between the fibrinousness and the perfection of the blood, in the facts that there is little or no fibrin in the blood of the fetus, none in the egg, none in the chyme, and less in the blood of the carnivora (who feed on it) than in that of the herbivora.

“Some of these facts, derived from very different sources, appear quite inexplicable on the theory that fibrin is essential to the progressive development of the tissues; and the opposite inference seems unavoidable, that it must be considered an excrementitious product, derived from the waste of the tissues or the oxidation of the blood, and in progress of elimination from the system. This conclusion, carried into the domain of pathology, would lead us to suppose that an augmented proportion of fibrin in the blood (whether occurring in active disease or within the limits of apparent health) can be taken as an indication only of increased labour and waste in certain elements of the body, not of an increased development in the resources and nutrition of the blood. And on the same grounds it would appear that a super-fibrination of the blood, in acute inflammatory diseases, must be regarded as a consequence and effect of those diseases, not as their cause, and not as a primary affection.”

If we add to all this, so well expressed by Mr. Simon, the *fact* observed by Dr. Brown-Sequard and verified by myself, that when defibrinated blood is made artificially to circulate through the limbs, trunk, or head of a recently dead animal, it emanates from the veins charged with fibrin, we find strong corroborative evidence in favour of the view that it is, in truth, a material of an excrementitious nature. The same notion is strongly supported by the further fact that the fibrin is destroyed in the blood passing through the liver and kidneys.

Physiologically, therefore, it appears that blood is rendered more suitable for the purposes of transfusion by being deprived of its fibrin. From a surgical point of view, of course, defibrinated blood has immense advantages. No one who has not made experiments on the subject can fancy the difficulty of transferring blood from one creature to another without its coagulating. To defibrinate the blood previous to its injection disarms the operation of more than half its difficulties and dangers. There is no occasion for undue haste; there is no risk of embolism from clots; the serum, the salts, and the blood discs, all the vivifying elements, in short, are present; nothing is left out save what is physiologically useless, if not injurious and surgically dangerous.

There is but one objection which can reasonably be urged against defibrination, and this is the time lost in doing it. Transfusion is generally an operation of emergency; there is, it is urged, no time for stirring the blood and straining it. To meet this I should propose that every case containing the transfusion apparatus should be provided with a solution of phosphate of soda, so that, according to the plan of Dr. Braxton Hicks,^a it might be added and coagulation prevented if it appeared that the patient's life might be lost during the few minutes lost in defibrinating.

As regards the advantages of omitting the fibrin from the blood, the experiments of Panum are important and indeed conclusive. He showed that, if clots of any size enter the vein, death occurs either during or immediately after the operation from obstruction of the pulmonary artery. If death is not immediate it may supervene later, owing to embolism produced at some point in the circulation.

The researches of Brown-Sequard and Panum have, therefore, a great practical value, inasmuch as they demonstrate that for purposes of transfusion fibrin is not an essential part of the blood. It is, as a matter of fact, reproduced in forty-eight hours, and its

^a Dr. Braxton Hicks (*Guy's Hospital Reports*, Vol. xiv., p. 1) recommends a solution of phosphate of soda of the same specific gravity as the blood. He says:—"I would propose that three ounces of fresh phosphate be dissolved in a pint of water; some of this will crystallize out, but when required for use it will rapidly re-dissolve if immersed in warm water at 100° Fah. The proportion of the solution to the blood employed should be about one-fourth."

Neudörfér recommends the addition of two grammes of bicarbonate of soda dissolved in from thirty to forty-five grammes of a solution of albumen or sugar to every 120 grammes of blood. It is obviously better, unless the circumstances very urgently call for it, to avoid the introduction of such substances.

absence exercises little or no influence on the quantity of urea evacuated.*

The case in which I some years ago performed the operation of transfusion in a patient suffering from tetanus presented some features of interest which make it worth relating. Although I cannot say that it seemed to exercise any beneficial effect upon the complaint, or even to prolong the patient's life, it certainly gave relief from a very distressing symptom. It was quite impossible to induce the patient to swallow anything, so great was the spasm of the gullet; enemata were instantly ejected. It was most piteous to hear her constantly crying through her teeth, clenched upon a piece of cork—"Save my life, save my life if you can; I am dying of hunger and thirst." An hour after the operation the sister in charge observed that she ceased to complain of hunger or thirst. The spasms did not, however, diminish in either frequency or intensity. As the patient was quite conscious during the performance of the operation, she was able to describe accurately her sensations as the blood was thrown into her veins. The following is a statement of her case, as noticed at the time by Dr. W. Dudley White, then residing in Jervis-street Hospital:—

Mary Anne Dooley, aged fourteen years, was admitted to Jervis-street Hospital on March 27th, 1865, with a contused and lacerated wound of the right hand, which had been caught between the rollers of a paper mill. The little finger was completely destroyed, and the rest of the hand severely injured. Up to April 16th the patient made favourable progress, and on that morning was up and about, but complained of weakness, and had no inclination for breakfast. In the course of the forenoon the sister in charge perceived the peculiar expression about the eyes and brow, which she recognized as tetanic. Very active treatment, with belladonna and tobacco stupes to the injured hand and limb, was adopted from the first. The following mixture was administered in increasing doses, until very marked symptoms (great dilatation of the pupil and delirium) were produced:—

R. *Atropiæ valerianatis*, gr. i.
 Ammoniæ valerianatis, gr. x.
 Aquæ, ʒ vi.

M. et solve.

* Panum Experimentelle Untersuchungen über der Transfusion, Transplantation oder Substitution des Blutes, &c. Virchow's Archiv, xxvii., p. 249.

60 minims by measure every two hours until effects on the pupil become marked.

A blister was applied round the arm on the side injured. This, as well as the hand, was stupefied with the infusion of tobacco.

All along nutriment was given as frequently and abundantly as the patient could bear or take.

On the second and third day the pulse rose to 140; opisthotonos became distressing; ice was applied along the spine, and chloroform administered with but very temporary benefit. After each time of its administration, however, it was possible to give beef-tea.

On the 19th the spasms were very frequent, occurring regularly every seven minutes, with a severe one every three hours; patient complained piteously of hunger and thirst; was absolutely unable to take nutriment by the mouth, and instantly rejected enemata.

On the following day she was apparently sinking from inanition, constantly saying that she was dying of hunger and thirst.

With the consent of my colleagues, Drs. Forrest and Tyrrell, I then performed transfusion. The patient was quite calm and collected; all delirium had ceased, the atropia having been suspended in consequence of the total inability to swallow.

Dr. Tyrrell took blood from my left arm to the amount of twelve ounces; this was defibrinated by stirring, strained through muslin, and kept at the proper temperature by allowing the vessel containing it to sit in warm water. It was then thrown into the corresponding vein in the patient's left arm. I had no difficulty in finding the vein and opening it. When I had introduced the nozzle Dr. Tyrrell carefully worked the syringe, which I at that time used, and by steady, gentle strokes, the entire of the blood was injected. Every two or three minutes an interval was given, and the patient asked how she felt. She expressed herself as feeling an agreeable sensation, an undefined sensation of warmth pervading her.

An hour after her sensations of hunger and thirst were quite allayed.

The operation had no effect whatever on the spasms: it did not seem to exercise the slightest control over the course of the complaint. On Friday (21st) the patient died, without pain, and quite conscious to the last.

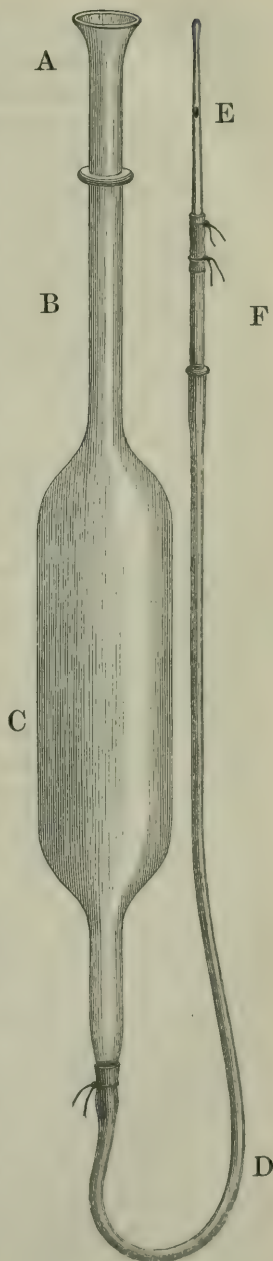
The apparatus which I now recommend for the injection of defibrinated blood or any other fluid into the veins is very simple. It has the advantage of dispensing with the use of the syringe, an

instrument ill adapted to operations of emergency, as the piston has often ceased to be air-tight just when it is necessary that it should be completely so. In its stead I use a strong glass pipette, which, with the pressure of air from the mouth, is quite sufficient to force in the blood.

The accompanying wood-cut will explain this simple apparatus. A B C represents a glass pipette, D Indian-rubber tube, and E a silver canula or nozzle, near to which a piece of glass tube (F) is inserted and adapted by Indian-rubber tubing.

The orifice (A) of the pipette should be of such a size that it may be closed by the thumb and slightly funnel-shaped, so that the fluid may be readily poured into it from a small jug or ewer. The stem (B) should be about half an inch in diameter. The bulb (C) should be capable of containing about six ounces^a of fluid, and may be graduated.

The lower end of the pipette should taper, so as to be very easily inserted into the Indian-rubber tube (D). The glass tube (F) near the nozzle should also be of a size just adapted to that of the tube which should be tied upon it. The silver nozzle^b (E) should be neatly made, probe-pointed, and at the base the size of No. 6 catheter; there is no opening at the end of it, but an eye or side opening at some little distance from the probe point.



^a Statistics prove that in the majority of the successful cases of transfusion recorded, there has not been more than from four to six ounces of blood injected.

^b These nozzles have been very neatly constructed for me by Messrs. O'Neill and Thompson, surgical instrument makers, 7, Henry-street, and 25, Nassau-street, Dublin, from whom cases containing all the necessary instruments for transfusion as here described may be obtained.

Besides this instrument every transfusion case should contain—1st, Bandage, lint, and lancets; 2nd, Glass stirring rod, and piece of muslin (both perfectly clean) for straining the blood; 3rd, Small sharp-pointed scalpel or tenotomy knife, forceps, scissors, and straight needles; 4th, Thermometer; 5th, A small clip or spring forceps to catch the Indian-rubber tube—this is simpler than a stop-cock, as well as lighter and more manageable; and 6thly, Some fine wire for securing the Indian-rubber tube at different points. This is more convenient for the purpose than ligature silk.

There is no real difficulty in the operation, but there is much nicety, and its success depends upon close attention to a number of apparently trifling details. Besides the things above mentioned nothing is needed but what can be at hand even in the dwelling of an humble patient, viz., some warm water, a basin, some bowls, a small jug or ewer.

The blood to be injected is usually obtained from the husband or some healthy relative or friend. In hospital, students are always found ready generously to offer their's. It should be drawn from a good-sized opening in the vein into a perfectly clean bowl or finger glass, previously rinsed with hot water. It is to be stirred as it flows into the bowl, with a glass rod, also perfectly clean; as soon as the shreds of fibrin are found clinging on the end of the rod (this occurs in five or six minutes) the blood is to be strained through muslin, previously dipt in boiling water, into a second vessel. This second vessel should be kept sitting in a basin of hot water at a temperature, ascertained by the thermometer, of about 105°. The straining of the blood gets rid not only of the clots of fibrin, but of the froth which has come from the stirring, which should be brisk.

The blood is now ready for use, and has only to be maintained for a little time at the due temperature. This is a matter of detail, varying with the heat of the season and apartment.

The pipette is next got ready, that is to say, the blood is sucked up into it until the bulb is filled; the tube and nozzle are adjusted to it, and the spring clip put on the tube so as to prevent the escape of the blood. When filled and ready it may be placed in a jug or any other deep vessel containing water of the proper temperature. If standing erect, with the spring on the tube and the nozzle elevated, no blood can escape, and it remains in such a condition that there is no occasion for any extreme haste in the further steps of the proceeding; there is no fear of its cooling too much in a moderate time, nor of its coagulating.

The next part of the proceeding is to open a vein in the patient. This may be done at the back of the hand or foot, but perhaps best at the bend of the elbow. The skin over the vein is pinched up between the forefinger and thumb of the left hand of the operator. This raises the skin only, and when it is transfixed and divided to a moderate extent the vein is usually seen crossing the cut; if not visible, we must press along the skin from below, so as to squeeze enough of blood into the vein to make it visible. When once seen a needle should be passed across underneath it; this is only as a mark, for if the vein contracts from exposure to cold air it is not easy to find it again. If necessary the vein may be slightly raised from the surrounding adipose tissue before opening it. This is done by taking hold of it in a fine pair of forceps, and making a nick in it with the scalpel. The pipette and tube are then brought, and the probe point of the silver nozzle is introduced into the vein, but not so far as the eyelet hole in the side of the canula. Before pushing it in so far as that, the spring clip is taken from the tube, and the blood allowed to descend and emerge from the eyelet hole, expelling all air before it. The canula is then pushed in, into the vein, and the blood allowed to flow on. The weight of the column, which may be increased by raising the pipette, is usually sufficient, but if not the mouth can make sufficient atmospheric pressure to ensure the passage of the fluid into the vein. When all the blood in the bulb has been thrown in, it can be re-filled by pouring in more without uncoupling it from the tube, but it is also easy, if there is not a ewer of convenient size, to take off the pipette and suck up the blood into it as at first. When the thumb is placed on the upper orifice it is controlled so that it can at once be re-adjusted to the tube. While the injection is going on the operator holds the Indian-rubber tubing between the forefinger and thumb of his right hand, close to the piece of glass tube; if any bubble is seen in the glass tube its passage into the vein can be instantly arrested by pinching the Indian-rubber tubing.

The apparatus just described has the merit of being very simple, and always in order; the use of it and of defibrinated blood makes the operation of such easy performance that I should venture to hope it will now be more frequently had recourse to than formerly. It is obvious that this apparatus is equally applicable for the injection of saline solutions into the blood vessels, a mode of treatment in cholera and other complaints well worthy of a more extended trial than it has as yet received.

ART. XI.—*On Santonin, and its Detection in the Urine.* By
WALTER G. SMITH, M.B.; Fellow and Censor of the College
of Physicians; Assistant Physician to the Adelaide Hospital.

TWO singular effects are known to result from the administration of santonin in moderate doses, viz., visual derangements, and a peculiar alteration in the colour of the urine. It is with the latter phenomenon that I am at present most immediately concerned, but the effect on the vision is so curious and, I believe, unique, that it is worth while to add another observation in confirmation of the reality of this remarkable symptom. On the 30th of August, a bright clear day, I took 5 grs. of pure white santonin half an hour after luncheon. While engaged in reading some three hours after, I became gradually conscious of a yellowish tint on the paper, and fancied that there must be a yellow haze in the air. My own hands and the complexions of others appeared of a sallow, unhealthy colour, and the evening sky, which was really of a pale lavender tint, seemed to be of a decided light green. Vision was not perfectly distinct for some hours, and was accompanied by a certain vagueness of definition. This effect has followed each time after a dose of 4 or 5 grs. of santonin, whether of the white or yellow modification, and is in harmony with Rose's observations. I felt prostrated and nauseated in the evening, was chilly and unable to sit up writing, but after the night's rest all unpleasantness disappeared. Larger doses have produced weariness and a sense of prostration, which continued for five days (Giacomini. Rose). Compare Exper. viii.

A boy, aged fourteen, subject to frequent epileptic fits, was ordered santonin powders on suspicion of the presence of worms. An hour after taking the first dose of 5 grs. his stomach was inclined to get sick, and he told the nurse that "her face looked very yellow," and surrounding objects assumed the same hue. After he had vomited he fancied that every object round him was coloured green. This effect lasted for twelve or thirteen hours. Similar results followed twice on repeating the dose, and each time appeared shortly after taking the drug. The occurrence of this visual disturbance, which was first described by Mr. Spencer Wells, has now been attested by various observers, and from E. Rose's investigations would seem to constantly follow a full dose of santonin. The patient sees, as it were, through coloured spectacles,

and the colouration of surrounding objects is variously noted as yellow, orange, green, sometimes blue, and even red if the dose be very large. Straw-yellow is the prevailing colour, but the apparent colour of objects alters at different times after the ingestion of the santonin, and varies with the dose. A young man had yellow vision after a 5 gr. dose. He repeated the dose in 36 minutes, and then saw objects of a red colour; half an hour after orange vision set in, changing finally to yellow (Martini). The colouration in some individuals is intermittent, and seldom or never lasts more than one day. Different opinions are held as to the cause of this xanthopsia or yellow vision, which fall under two heads. Some maintain, with Giovanni, that it is due to an actual colouration of the transparent media of the eye by the yellow substance circulating in the blood, which is derived from the oxidation of the santonin into santonicin or santonein. But, though Giovanni asserts that santonin previously turned yellow by exposure will not affect the perception of colours, the absence of any visible change in the sclerotic or in the humours of the eye speaks against his view, independently of the positive evidence as to the disturbing effect of the yellow variety. A more plausible interpretation is given by Rose, who claims to have shown that the violet and then yellow colour which covers the field of vision during the use of santonin is due, not to any change in the actual colour of the media by which the rays of light reach the retina, but to an altered perceptivity in the nervous organ of vision itself. In fact, a sort of temporary Daltonism is set up, the proximate cause of which is probably disturbance of the retina, consequent on some alteration in its circulation. On the whole, it is most probable that two causes concur in the production of the ocular phenomena—one, belonging to the chemical change of colour which produces a temporary effect and determines yellow or greenish-yellow vision; the other, resulting from the consecutive nervous sensation which gives rise to the production of complementary colours, the disorder of innervation being the more important (Mialhe). It is remarkable that this yellow colouration of vision is not observed in subjects affected with atrophy of the retinal arteries, or in chronic choroiditis attended with resorption of pigment.

Passing this by let us now examine the phenomena connected with the urine of persons to whom santonin has been given. Many substances are known which, when taken into the stomach, visibly affect the characters of the urinary secretion in their transit through

the system, and a number of vegetable colouring matters seem to pass unaltered into the urine. For example, madder communicates a red tint to the urine, and under the use of senna this secretion acquires the property of being reddened by ammonia. When rhubarb is administered it tinges the urine yellow or red, according as the reaction of that fluid is acid or alkaline. Carbolic acid, absorbed by the stomach, causes the urine to become greenish, and when freely applied to the skin has frequently developed a smoky or even black colour. Since the isolation of santonin by Kahler, forty years ago, it has almost entirely superseded santonica, and within the last twenty years is largely and increasingly employed as a safe and efficient vermifuge. Various observers had noticed that the urine of persons under the influence of santonin is tinged of a saffron-yellow or greenish colour, and Giovanni and Ambroise state that it often induces apparent hematuria. The urine of some of the lower animals, *e.g.*, rabbits, is similarly affected. As in the case of rhubarb, the shade of colour depends, no doubt, on the reaction of the urine, which is coloured yellow when acid, and purplish-red when alkaline.

My attention was accidentally drawn to the more particular examination of the urine by a case which occurred in the Adelaide Hospital. A boy, aged five, was given 4 grs. of white santonin on the evening of August 18th. Next morning the urine, which had been kept in a tall glass vessel, was of a bright pinkish red. Though the reaction of the urine was noted at the time as being acid, an error must, I think, have crept into the notes, for it would be difficult on any other supposition to explain the cause of the red colour. On the evening of August 19th the boy got another 4 gr. dose. The urine passed soon after was observed, on the succeeding day, to be of a greenish-yellow colour. A few drops of liq. ammoniæ were added, and immediately a clear red tint was produced. This reaction was new to me at the time, and is not referred to in any of the standard works in English, except Stillé and Ringer, but it was first noticed eleven years ago by E. Rose (*Virchow's Arch.*, xvi., xviii.) Dr. M'Alister has informed me that he once saw red urine follow the administration of 2 grs. of white santonin to a child four years of age. In order to obtain sufficient material for examination I made a few experiments on myself, which I will briefly cite before entering into further details.

Experiment I.—Took 5 grs. of santonin half an hour after a meal, without previously emptying the bladder. No indication of

santonin in the urine till fifty minutes had elapsed. The urine continued to respond to the alkali test for forty-eight hours. Yellow vision was produced.

In all the succeeding experiments the bladder was previously emptied.

Exper. II.—Took 4 grs. of powdered santonin in bread three and a half hours after lunch. Colour reaction faintly developed in ten minutes. Elimination completed in about forty-eight hours.

Exper. III.—Took 3 grs. of powdered santonin five hours after breakfast. No evidence of santonin was obtained in the urine after thirty-seven minutes. The observation was then interrupted, but in the afternoon the test-reaction was well marked, and was perceptible for more than thirty hours.

Exper. IV.—Took 4 grs. of powdered santonin before breakfast; appeared in urine in twenty minutes, and continued to manifest its presence for about forty-eight hours.

Exper. V.—Took 1 gr. in claret four and a half hours after breakfast; evidence of its presence in an hour; eliminated in about thirty hours.

These experiments were all made with pure white santonin: the succeeding observations were made on yellow santonin. The santonin was finely pulverized in an agate mortar, and thoroughly insolated by exposure to direct sunlight for two or three days, and also to diffused light for some time. The drug was spread in a thin layer between two plates of glass, and thoroughly mixed and stirred several times.

Exper. VI.—Took 3 grs. of santonin before breakfast; no evidence of its presence till thirty-five minutes had passed; eliminated in forty hours.

Exper. VII.—Took 5 grs. in sherry three hours after breakfast. In fifteen minutes a slight test-colouration, becoming distinct in twenty minutes, and remaining manifest for thirty-four hours, when the observation was interrupted.

Exper. VIII.—Took $6\frac{1}{2}$ grs. in sherry an hour and a half after breakfast. In twenty minutes first faint indication in the urine; eliminated in about forty-eight hours. Vision disturbed during the day by a certain indistinctness and haziness, but no decided yellow sight. Marked lassitude and weariness, especially in the lower extremities, continuing for two days.

A small quantity of saliva was several times tested, but always with negative results. It is stated that the serum of the blood is

coloured yellow by a large dose. The colour of the urine was in all cases greenish-yellow, sometimes approaching a light saffron tint, and the greenish hue is best seen by looking obliquely across the surface of the fluid. It bears no small resemblance to the urine of a person slightly jaundiced, and, like it, stains linen of a persistent light yellow. In two other respects also it curiously conforms to bilious urine. When nitric acid is dropped on a small quantity of the urine, a distinct purplish colour is brought out, which, however, is evanescent. Again, when sulphuric acid is added drop by drop it develops a reddish-brown colour, changing to a deeper brown. I need scarcely add that no such effects were produced on the urine, except when under the influence of santonin. The sulphuric acid reaction is even less ambiguous than that with nitric acid, which might affect the colouring matter of normal urine somewhat similarly. These reactions, viewed in connexion with the altered colour of the urine, show that it would be quite possible, from a hasty examination, to make an erroneous diagnosis of bile in the urine, and point to a possible fallacy in regard to the two most common clinical tests for bile.

The test for the detection of santonin in the urine, which has been already alluded to, is a very striking one, and may be noticed both as to its character and its sensitiveness. It simply consists in the addition of an alkali to the urine, when a fine cherry red or crimson colour will immediately be developed, according to the amount of santonin present. The urine will respond to potash, soda, or ammonia, and also to lime or baryta water. If a globule of potassium be dropped on the urine, a bright red track is left wherever the burning metal skims along the surface. At first ammonia was employed, and the colour is well brought out by pouring a few drops of liquor ammoniæ down the side of the test-tube, so as to float on the urine, when the red zone will appear sharply marked at the line of junction of the two liquids. But potash was afterwards found to be a more delicate re-agent, and is better suited for general use. The red alkaline fluid is not bleached or altered by boiling, but the colour is at once destroyed by any acid, even carbonic acid gas. The subsequent addition of alkali restores the colour as before; hence it may be inferred that the colouring substance is not impaired or broken up by acids. Bicarbonate of sodium produced no immediate change, but on boiling for some time the reddish tint was gradually developed, and was discharged by continued boiling. Carbonate of sodium afforded similar results,

except that it required longer boiling before the colour was discharged. Phosphate of sodium gave no result. The red coloured stratum soon subsides to the lower part of the test-tube, carried down by the precipitated phosphates. Prolonged exposure to light in contact with excess of alkali bleaches out the colour, and chlorine at once dissipates it. When we remember the sparing solubility of santonin, one part requiring 5,000 parts of water at 17.5° C., the delicacy of the test is sufficiently apparent from the facility with which santonin is detected in the urine within ten minutes after 4 grs. were taken (Exper. II.), and within an hour after but 1 gr. was taken (Exper. V.) In Exper. IV. the urine voided twenty-four hours after the dose gave a decided red colour with liq. potassæ, even when diluted with three parts of water.

For ordinary doses of from 3 to 6 grs. about two days are required for its elimination, and it is to be remarked that the urinary colouration and reaction to the alkali test are more persistent than the phenomena connected with vision.

When the red liquid is examined with the spectroscope, the red, orange, and yellow rays are transmitted, while the blue end of the spectrum is absorbed. In a more dilute state the red and blue rays are transmitted, and the centre of the spectrum is stopped. No characteristic bands are produced. It now remains to inquire into the nature of the colouring material found in the urine, and to ascertain its behaviour with re-agents as an aid to any further steps towards its isolation. On these points the results are as yet incomplete. After a preliminary experiment, the following process was adopted at the suggestion of Dr. Emerson Reynolds, to whom I am indebted for much assistance. About a pint of urine, passed on Sept. 2nd, after taking 4 grs. of santonin the preceding evening (Exper. II.), was treated with neutral acetate of lead, avoiding excess, and then filtered. To the filtrate neutralized by potash basic acetate of lead was added so long as any precipitate was formed, and until the fluid became colourless. Neutralized, filtered, and washed. The yellow precipitate was transferred to a beaker, and decomposed by the cautious addition of dilute sulphuric acid; spirit of wine was added, and the beaker set aside for twenty-four hours. Filtered; removed excess of sulphuric acid by barytic water, and filtered. The clear fluid now gave the pink reaction distinctly with potash, but ammonia had no longer any effect. The colouring matter, therefore, was evidently set free, but the quantity at command was too small to admit of a more minute examination.

The red alkaline filtrate gave a bulky precipitate with alum, but when this precipitate was filtered off, neither the precipitate nor the filtrate any longer afforded a trace of colour with potash. The liberated colouring substance does not seem to enter into combination with nitrate of silver, nor is it visibly affected by corrosive sublimate, sulphocyanide of potassium, chloride of gold, or bichromate of potassium. With persalts of iron it gives a permanent rich brown colour. From these experiments it may be gathered that the colouring material is tolerably stable, that it combines with *basic* acetate of lead, and can still be detected in that state of union by the potash test. It is highly probable that the colour-developing substance is a feeble acid derived from the intra-vascular oxidation of the santonin, and in support of such a view an experiment of Mialhe may be brought forward.

When santonin, in powder, is submitted to boiling nitric acid, a product is obtained which, after saturation, gives, with water, a greenish-yellow solution, analogous in appearance to that which urine assumes under the influence of santonin. This liquid, when treated with an alkaline base, immediately develops a deep orange-red colouration similar to that which urine secreted after the ingestion of santonin assumes with alkalis. This experiment I have repeated on the white and yellow forms of santonin, and with corresponding results, yet, though the reddened alkaline fluid gave an abundant yellow precipitate with basic acetate of lead, neither the precipitate nor its filtrate were any longer coloured by potash. Neither could any colouration be obtained on decomposing the lead precipitate with sulphuric acid, and testing the fluid decanted from the sulphate of lead with potash. If in these respects the artificial colouring matter fails in its analogy to the natural colouring substance in santonic urine, still Mialhe's experiment may be regarded as indicating the direction in which to seek for the cause of the natural phenomenon.

ART. XII.—*Moral Mania*. By J. H. BALFOUR BROWNE, Esq.,
Barrister-at-Law.

THAT the one black sheep which is within the fold of a respectable household should be white-washed, may, to piebald brothers and sisters, seem a desirable thing. That a family moving in good society, and living in a good street, should, in the event of one of

its members committing a crime, have recourse to the family physician rather than to the police, and should look upon the act as a symptom of disease and not as a crime in the true sense of the word, seems a very natural proceeding. For a long time insanity was looked upon as the work of God's hand, while, even at the present day, the devil is regarded as the mechanist of crime. If, then, a family has an opportunity of mistaking the hand of the devil for that of God, it will probably embrace it. Many a one when asked, like Sam Weller, if he can see the individual who was guilty of contempt of court, and knows that that individual is a relation, and had laid himself open to punishment, will look at the ceiling, and say "No!" Heaven knows that the grandest things upon earth are those dear home-eyes which won't see our faults—those dear lips that are "no thoroughfare" for reproaches, and those dear heads which are armories full of defences of our errors, which would fain find a leaning to virtue in all our vices, and the mental darkness caused by the shadow of God's hand in that night of the moral life in which the devil rides. But although friends may be break-waters about the home-harbour, it is the duty of a Government to punish crime, and in order to do so it is necessary to distinguish crime from insanity.

Is there, then, such a disease as moral mania?—a disease the symptom of which is crime—and if there is such a disease how is it to be distinguished from immorality? Pinel was the first who asserted that there were "many maniacs who betrayed no lesion whatever of the understanding, but were under the dominion of instinctive and abstract fury, as if the affective faculties alone had sustained injury;" and very many writers since his time have distinguished between intellectual and moral insanity. Some have argued that this disease is exclusively confined to the moral sense, that it may co-exist with a perfectly healthy condition of every other faculty, and that the only symptom which manifests the presence of disease is depravity in a somewhat exceptional degree! That twenty convictions would prove a man mad, the law has as yet denied. That if the disease is manifested by no other symptoms than the commission of criminal acts, the individual shall be liable to the consequences of those acts, the law has upon more than one occasion asserted; and although many loud voices have been raised against the law on account of that denial and that assertion, the principle laid down seems to us to be sound. Nay, further, although we admit that crime is in many cases a sign of

the presence of disease, and although we think that in most cases in which it is so, the history of the individual, and the presence of insanity or nervous disease in the parents will establish the fact of moral insanity in the individual under examination; we are of opinion that only on very rare occasions should moral insanity stand between the individual and the consequences of his criminal acts, for it seems to us certain that punishment is in most cases one of the means of cure, and that moral maniacs may be restrained from criminal acts by an adequate system of discipline!

The philosophy of the subject seems somewhat defective. We find frequent assertions that this disease consists in a morbid perversion of natural feelings, or habits, or moral dispositions, and that it is unaccompanied by any lesion of the intellect; that it is a disease of the moral sense, and various other assertions of similar import. Writers have not taken the trouble to ascertain, in the first instance, whether there be a moral sense or no; they have not endeavoured to discover whether it is possible that reason, when directed in one particular direction, can be affected with disease, while in other directions it can be exercised under all the conditions of health. It is an easy thing to take for granted, and then to assume as proved.

Ethics is the science of the laws of our actions looked at with regard to their morality or immorality, and presupposes a knowledge of man as a moral agent. If, however, our ideas of right and wrong are formed in connexion with the ideas of reward or punishment—if our dislike and disparagement of certain actions, and our approbation and praise of other actions, is founded on our belief that in the one case the individual committing the action should be punished, and that in the other case he should not, it is evident that our moral distinctions have an intellectual origin, and that any such phrases as moral sense, or conscience as distinguished from ordinary intellectual function, are apt to deceive, and any distinction between moral and intellectual insanity is unphilosophical. This is not the place to consider whether this is a true statement of the fact or not. Man, individually, ought to make all his actions perfect. Government has to be content with a moral code that will *do*. Government cannot enact the whole moral law as laid down in books of ethics or in great human hearts. All it can attempt is to make men free to be good if they will by restraining acts of violence. Expediency is essentially the science of government, and as that is the case it will be sufficient if we, in this place,

point out what acts government ought to recognize as moral, what acts government ought to punish as immoral, and in what way a somewhat rough and ready morality may be applied to the solution of the questions which arise in reference to the so-called moral insanity.

That for all governmental purposes "good" may be defined as "happiness," and "bad" as "misery;" and that as each person's happiness is in this view regarded as good to that person, the general happiness must be regarded as a good to the aggregate of persons seems to us to be true. That those actions which tend to the happiness of the individual, and which do not interfere with any enjoyment of other persons, should be regarded by the legislature as moral, and that such acts as lead either directly or indirectly to the misery of the individual, or which, while ministering to the happiness of the individual, are calculated to take, either directly or indirectly, from the happiness of others, should be regarded as immoral, seems to us fair. The government, be it representative or not, is a trustee for the community. The object of the trust is the attainment of the greatest amount of happiness to the *cestui que trust*, and one of the means adopted in this country for the attainment of that object is the enactment of a code of laws, which declares that certain acts—believed to militate against the public good—shall be punished in case they are committed, not because government wishes to punish crime that has been committed, but because the invariable connexion between an act and a serious disadvantage to the actor is likely to lead to the discontinuance of the act, and in that way lead to the greater happiness of the community.

The various mental peculiarities, some of degree, some of kind, which distinguish man from the brute, seem to be—1. The greater ability of the former to profit by experience, to get the essence out of facts, to learn something more from his faults and failures. 2. The more perfect means of communication which man possesses in grammatically constructed languages, and a much more complete repertory of the lower gestures, of body, and the higher gestures, of expression. 3. The more intimate relations of man to man than those which exists between animal and animal, which lead to an infinitely great play of feeling, to voluntary ornamentation, which is not directly connected with material well-being, and to which may be referred the tendency to associate, which is characteristic of man, and which induced Aristotle to

call him a political being. But of these three characteristics of humanity the first seems to us, perhaps, the most important, and possibly the characteristic to which the development of the other two peculiarities might be referred. With regard to the power which is inherent in the being of man, of availing himself of surrounding phenomena, of profiting largely by experience, and of advancing through failure to success, through pain to pleasure, it must be borne in mind that animals have the same power, although in a less degree. Monkeys that have once burned their lips in swallowing hot liquids afterwards wait with patience until they are cooled.^a Every one knows that by means of rewards and punishments dogs, cats, canaries, and fleas can be taught many things. But there is a certain degree in which this power or capacity is possessed by different animals, and beyond that power punishment is thrown away, or rather its effects are manifested, not in the improvement of the individual on whom the punishment is inflicted, but in deterioration both of the individual punishing and the individual punished. The doctrine of all true educational or reformatory punishment is to punish as long as the individual and class to which he belongs, and on whom the example will operate most powerfully as a deterrent, have capacity sufficient directly to concatenate the suffering with the offence, and to understand how they may avoid the commission of a like crime. Any infliction of punishment under circumstances other than those just alluded to is not only inefficacious, but tends to diminish the aggregate happiness of mankind, and is to that extent a breach of the trust reposed in the government of the country. It will, therefore, be understood that repeated convictions on account of the same crime would naturally lead to a suspicion of an amount of incapacity which would justify the law in exempting an individual from criminal consequences; and while such an amount of incapacity is proved in reference to acts occurring in the life of the individual, other than those which have come under the cognizance of courts of law, the presumption is strengthened; and further, if in conjunction with these circumstances it is found, upon inquiry and examination, that there is an inherited tendency to insanity, or malformation of the skull—if the history of the case is such as to lead a physician to suppose that it is not impossible that the mind may be diseased, in such a case it seems to us that the law would do well to admit the existence of moral mania, and exempt the

^a Bennet—*Wanderings in New S. Wales*, Vol. ii., p. 158.

individual from the legal consequences of criminal acts. But the law is asked to do more: it is asked to believe that persons who, while "labouring under this disorder, are," according to Pritchard, "capable of reasoning or supporting an argument on any subject within their sphere of knowledge that may be presented to them—and they often display great ingenuity in giving reasons for their eccentric conduct, and in accounting for and justifying the state of moral feeling under which they appear to exist"^a—are in no case fit objects for punishment. It is owing to such demands that the criminal courts of this country have been less willing to admit moral insanity as a bar to punishment than they would otherwise have been, for those persons who have gone so far as to assert that a morbid perversion of sentiments, as manifested by repeated acts of crime, should in all cases be treated as disease, have not hesitated to regard all crime as a form of morbidity, instead of regarding it, in its truest sense, as one of the conditions of the health of a community. That such pretensions should have made lawyers and legislators sceptical as to the authority to be attached in questions of this kind to the evidence of medical gentlemen, was not to be wondered at. But the two questions—Is there moral insanity? and if it is proved, how far should law recognize it as depriving the individual of that capacity which is commensurate with responsibility?—are distinct. As to its existence let us examine the evidence.

1. It is asserted by many of the ablest writers upon insanity.
2. There are many cases in which the motiveless character of the act done, the past history of the individual, the carelessness as to whether the commission of the crime is discovered or not, lead to a belief in the existence of insanity. We almost invariably find that this form of insanity is said to be accompanied by what are called "depraved impulses," and that it is asserted by many that "defective volition" and "perverted emotions" are mental symptoms of the presence of this form of disease. Now, as ordinary criminals have depraved impulses, as their wills must be defective to some extent, and as their reasoning powers must, through this deficiency, have led to their detection; as, further, the emotions of criminals are not generally of a very perfect human type, it would seem necessary clearly to understand what is meant by the assertions of those who pretend to know something about the subject. Before doing so we may state our belief that, through various circumstances to be afterwards

^a *Cyclopædia of Practical Medicine*, Art. *Insanity*, p. 826.

alluded to, a man may reason correctly concerning one set of phenomena, while he is incapacitated from reasoning concerning other sets. We find in ordinary life that Reason does not seem to be current coin, but a sort of local issue—that the very familiarity with the logical sequence in relation to a certain class of facts renders the individual unable to appreciate the same identical sequence in relation to facts of a different nature—that there seem to be men who can reason concerning the dry-bone facts of science, and are utterly unable to grasp the sappy facts of human science. A very limited knowledge of abnormal conditions will serve to convince one of the truth of the fact that this is much more prominent in connexion with disease. So that at this stage it would be wrong to deny that a mind, because it shows shrewdness and ingenuity with relation to many intellectual matters, may, at the same time, be unable to appreciate the relation of acts to personality looked at in their moral relation, just as there are many perfectly sane persons who can never come to regard virtue as an end in itself, which must ever be one of the crowning advantages of all true morality; for, as Mr. Mill observes, “there is this difference between it (the love of virtue for its own sake) and the love of money, of power, or of fame, that all these may, and often do, render the individual noxious to the other members of the society to which he belongs, whereas there is nothing which makes him so much a blessing to them as the cultivation of the disinterested love of virtue.”^a We proceed, then, to inquire what this “impulsive insanity” may be. As for the literature of the subject, so far as it has come under our notice, it is simple assertion, together with reports of some cases in which impulse was supposed to be present; and we may say here, that if medical gentlemen would, instead of clamouring for the recognition of irresistible insane impulses in courts of law, devote themselves to the proof of their existence, their time might be more profitably spent.

We know how intimately all our feelings are connected with thought, and how much thought is influenced by feelings. Well has Bacon said—“The light of the understanding is not a dry light, but drenched in the will and affections;” and it is well, in further explanation, to add that thought is, as it were, the skeleton of our mental life, while feelings and desires are the muscles and nerves which clothe it. Under such circumstances it is impossible to see in what way the one set of faculties, so to speak, can be

^a Utilitarianism, P. 57.

affected without the other. If a man does not know right from wrong he reasons badly. If he is unable to restrain desires by the leash of thought, or by fear of consequences, again he reasons badly. If the ordinary motives have no influence over the mind of an individual, we say he is a fool, or he is mad. Now there are very various ways in which a man may lose control over his actions. If you tickle the sole of a sensitive foot during sleep the leg is withdrawn by what is sometimes called "involuntary" retraction. If acts are repeated very often they become what is popularly called "second nature," that is to say, they become as involuntary as any of the actions which are performed during infancy, or as any of those habits which are the heritage of the race. Many actions in this way pass beyond the range of will or motives, for every act tends, by repetition, to become easier, and to pass out of the dominion of wilful choice into that of automatic origination. Every one knows the influence of habit. *Hamlet* says to his mother:—

"Refrain to-night,
And that shall lend a kind of easiness
To the next abstinence; the next more easy,
For use almost can change the stamp of nature."

One ethical philosopher has well said—"Do right, and trust to God to make it easy." So it is that many acts become so easy in the doing, require so little effort of consciousness, that they are said to be done unconsciously, or, in other words, out of the ordinary relation to thought in point of time. These acts we may call automatic or impulsive. The constant modified exercise of any of the muscles, will, in the time to come, tend to the same modified exercise under the influence of a comparatively insignificant exciting cause. And thus in all the manifestations of mind, whether they be connected with impressions on the senses, with the result of the processes of thought, or with one's actions as a moral agent, there is a liability to pass partially out of the power of will or motive—for we are using these two words in the same sense—a liability to recur under the influence of what would in time past have been an inadequate stimulus, and to become what may be called involuntary or impulsive. But this is much more speedily done under the influence of disease than in the conditions of health. The infinite variety of the actions that one is called upon, while in a healthy state, to perform, protects the individual from the effects that follow habitual action in one direction. But it can easily be understood that the presence of a delusion must greatly modify

many of the relations of motives to conduct; and the same effects will be produced by the influence of unrestrained habit, or of hereditary tendency. So powerful is hereditary tendency that we may be said to inherit ready-made habits; so powerful is it that a father may weaken the power of will, or weaken those powers by which men judge of motives, in his offspring. It is stated as a fact that Oxford, the regicide, believed that he was St. Paul, and that his grandfather had done the same. An interesting example of a hereditary propensity to steal—which descended from a real thief, who could refrain from pilfering when paid to do so, to his son and grandson—is given in Dr. Julius Steman's very excellent work on "Hereditary Disease," and other examples of a similar tendency to the reproduction of morbid propensities will be found in Mr. F. Hill's "Reports on Prisons." We find voices, features, even acquired skill, modified by the past, so that the handwriting of one individual member of a family has in some cases been found to resemble that of some ancestor whose writing he had never had an opportunity of seeing. All this seems to us to explain what we mean by the "depraved impulse," as present in many cases of moral insanity, and what, in this relation, we understand by defective volition. That disease has the power of withdrawing certain acts from the influence of will, and that in many cases it so much incapacitates the individual as to place him so thoroughly under the influence of one set of motives as to make any action arising therefrom rapid and unhesitating, is, we believe, the only true explanation which can be given of those diseased impulses which find place in the minds of the insane; and we are further inclined to interpret the apparently motiveless character which belongs to such impulsive actions as, in truth, due to the strength of the motive to which they owe their existence.

In this life our course is not a clear one. Duty is often difficult to do. We have the choice of Hercules at every turning point of our life. The more one knows of the possibilities of the future, the more careful will one be in fashioning the actualities of the present. And Shakespeare says—

"Rightly to be great
Is not to stir without great argument."

It is to the ignorant that choice is easy: it is to the wise that choice is difficult. A child finds no difficulty in choosing between a bank-note and a lollypop, and from the impulsive way which it grasps at the latter, we think it is uninfluenced by motives,

the fact being that it is influenced by the motives of actual enjoyment powerfully, and by those of remote contingency not at all. So it arises that an overwhelmingly powerful motive has, to the eyes of those who are in the habit of connecting choice of motives with struggle, the same appearance as no motive at all. Our idea of choice is the swaying of the scales, not the kicking of the beam by one of the scales; so that we have, in thought, connected actions which spring from a very strong motive with the expression "motiveless," because struggle, resistance, is a sign of humanity in the hands of cause, while yielding is the sign of inanimate matter under similar circumstances.

We believe that this explanation of an "irresistible impulse" is conformable to all the circumstances which attend their manifestation, as far as they have been accurately observed; and we further believe that, with such an explanation as the above, courts of law would, in connexion with the various cases, be satisfied of the existence of morbid impulses; and it would be admitted that persistency of criminal tendency, and the commission of criminal acts in spite of repeated punishments, and in spite of every human reason to believe that the connexion between future acts of crime and punishment would be invariable—all point to the existence of disease. A few cases are added to show that the real ground for exempting from punishment has not been sufficiently understood, and to illustrate the phenomena of this disease.

"An only son of a weak and indulgent mother, was encouraged in the gratification of every caprice and passion of which an untutored and violent temper was susceptible. The impetuosity of his disposition increased with his years. The money, with which he was lavishly supplied, removed every obstacle to the indulgence of his wild desires. Every instance of opposition roused him to acts of fury. He assailed his adversaries with the audacity of a savage, sought to reign by force, and was perpetually embroiled in disputes and quarrels. If a dog, a horse, or any other animal offended him, he instantly put it to death. If ever he went to a fête, or any other public meeting, he was sure to excite such tumults and quarrels as terminated in actual pugilistic encounters; and he generally left the scene with a bloody nose. This wayward youth, however, when unmoved by passion, possessed a perfectly sound judgment. When he became of age, he succeeded to the possession of an extensive domain. He proved himself fully

competent to the management of his estate, as well as to the discharge of his relative duties, and he even distinguished himself by acts of beneficence and compassion. Wounds, law suits, and pecuniary compensations were generally the consequences of his unhappy propensity to quarrel. But an act of notoriety put an end to his career of violence. Enraged with a woman who had used offensive language to him he precipitated her into a well. Prosecution was commenced against him; and on the deposition of a great many witnesses who gave evidence to his furious deportment, he was condemned to perpetual confinement in the Bicêtre."^a Although this case is recorded by Pinel, it seems to us anything but a satisfactory illustration of moral insanity; and it is somewhat strange that it has been quoted in that connexion by some more recent writers on the subject. "Strong passions," seem to us, all that is made out in the case just quoted; and if that plea is to open the door of a lunatic asylum instead of that of a prison, courts of law may shut their doors. That the strong passions were unrestrained in youth, that a defective education led to careless self-control, that the means of gratifying passions made them strong—just as in a country's economy, plentiful supply strengthens demand—that habit strengthened more and more what tendency had made in clay, seems all that can be gathered from the facts of this case. The same story might be told of many of those persons one meets each day and never excite a suspicion of insanity. We suspect that the money, of which he had much, and the domain, which was extensive, had something to do with his incarceration in the Bicêtre.

A case mentioned by Hoffbauer^b better illustrates what we understand by moral mania. It is a well known case, and we therefore content ourselves by referring to it. We epitomize a case given in an article upon the subject under consideration, in the *Medical Mirror*.^c

W. R., was twenty-seven years of age. He had been eight times in the house of correction. His father was an epileptic, and he himself had been subject to convulsions when teething, and at intervals during his after-life. He tortured animals, picked out the eyes of a kitten with a fork. He lied and stole. He was expelled from school as too bad to be kept. He afterwards consorted with the worst characters, was drunken, debauched,

^a Pinel Sur l'Aliénation Mentale, p. 156, s. 159.

^b Hoffbauer's Médecine Légale, s. 126, p. 132.

^c Notes on Moral Insanity, Medical Mirror, Vol. iv., No. XLVIII.

dishonest. He attempted, or pretended to attempt suicide. He was utterly false and untrustworthy. He delighted in torturing those patients who were, like himself, confined in the lunatic asylum, and who were too weak to resent injury with violence. He was indelicate in the presence of females, and attempted a rape on his mother and on his sister. Yet, with all, he was intelligent, exceedingly cunning, and while he was actually the victim of epileptic seizures, he was prone to feign fits, and did it with considerable ability. In spite of careful watching he repeatedly effected his escape. Was exceedingly vain; and, in the presence of some persons, seemed to be exceedingly devout. He was ingenious in excusing his errors; and, although exceedingly mischievous, was careful to avoid disagreeable consequences. All these facts indicate the presence of disease; and, we are inclined to believe, that the case above quoted is one in every respect typical of general moral mania; and yet it is not one in which it seems to us, looking at the function of government, as we have described it above, where even the presence of this morbid state should not protect from the consequences of criminal acts. In all the circumstances of the case we have partially described, two things are observable:—1. A fear of personal inconvenience, a dislike of ordinary punishments, and many of the ordinary motives of human nature—as self-aggrandizement, sexual indulgence, the praise of those whose praise is ordinarily thought of value, personal vanity, and the like; and, 2. An intelligence of such a high order as to enable him thoroughly to understand the relation between a found out crime and its punishment, as he invariably tried to conceal the commission of the criminal act by lies, hypocrisy, and various clever explanations. And either of those two conditions of health seems to us—where no incontrollable impulse is proved—to indicate a fit object for punishment. And the writer of the article from which the case is quoted, evidently, although a medical man, tends to the same opinion, for he says:^a “Humane and well devised punishment must follow all their (the morally insane) misdemeanours; and they must be made to feel that, in certain matters, subjection to a dominant system is an inevitable necessity. The gradual formation of habit is, above all things, to be aimed at.” It is quite evident that many such individuals exist amongst us, with a heritage, if not of actual disease, yet of accumulated crime, which is the clay in the hands of that potter, Time, of which insanity is made, who do not restrain their morbid impulses

^a Page 739.

on account of the fear of punishment. But because the law has, by various punishments, failed to make a man honest, to regard him when he again steals as exempted from punishment on account of the number of his crimes—and because it has failed, to do damage to its declaration by rendering the connexion of crime and punishment less invariable, seems absurd. It is just because this consequent and antecedent are not invariably and inevitably connected that some men commit crime, and that those who have a tendency to commit crime through strong passions, habit, or disease, are not restrained. It is true, there are some insane persons whom an invariable sequence will not teach *the* lesson of life, and whom the pain which is the invariable consequent of violently striking one's head purposely against a stone wall, will not teach to refrain from that act. When such a state of mind exists, whether it arises from imbecility or mania—intellectual or moral—it is absurd to punish. In most of the cases of moral mania which have been brought under our notice, the tendency to sin is, doubtless, due to disease; but it is not so strong that an absolute certainty of proximate suffering could not restrain from the commission of the criminal act—indeed in many cases it is not stronger than the tendency which exists in those persons that circumstances have brought to sin, and that habit has made criminals; and as it is for the latter class that laws are enacted, it seems to us, the former class are co-heirs with them in the advantages to be derived from the infliction of punishment. Another case may be quoted to illustrate this position; it is a case “where, with great natural shrewdness, general information, and gentlemanly manners, where no delusion or incongruity of thought can be detected, there exists an inveterate desire to torment and irritate those around: to enjoy the dissension and disputes which ensue, and to violate every rule of decency and delicacy by obscenities of look, word, and action, *when these objects can be accomplished without detection.*”^a We imagine that the case just quoted, and the following case, which we take from Prichard,^b prove that in many of the relations of the morally insane to the State, they may, for all the purposes of just governmental discipline, be regarded as sane; and that, in many respects, those who are afflicted with moral insanity must be treated in the same way as those in whom we can only discover moral turpitude. “Mr. H. P., had been for many years confined in a lunatic asylum, when, an

^a Crichton Institution Report for 1850, p. 26.

^b Cyclopædia of Practical Medicine, Art. Insanity, p. 834.

estate having devolved upon him by inheritance, it became necessary to subject him anew to an investigation. He was examined by several physicians, who were unanimous in the opinion that he was a lunatic; but a jury considered him to be of sound mind, attributing his peculiarities to eccentricity, and he was consequently set at liberty. The conduct of this individual was the most eccentric that can be imagined; he scarcely performed any action in the same manner as other men; and some of his habits, in which he obstinately persisted, were singularly filthy and disgusting. For every peculiar custom he had a quaint and often ludicrous reason to allege, which indicated a strong mixture of shrewdness and absurdity. It might have been barely possible to attribute all these peculiarities, as well as the morbid state of temper and affections, to singularity in natural character, and to the peculiar circumstances under which this person had been placed. But there was one conviction deeply fixed on his mind, which, though it might likewise be explained by the circumstances of his previous history, seemed to constitute an instance of maniacal delusion. Whenever any person, whom he understood to be a physician, attempted to feel his pulse, he recoiled with an expression of horror, and exclaimed—‘If you were to feel my pulse you would be lord paramount over me for the rest of my life.’” “The result has proved,” this author goes on to say, “that confinement is not always necessary in cases of this description. Mr. H. P. has remained at liberty for many years, and his conduct, though extremely singular, has been without injury to himself or others.”

This is one case, and many others might be collected in which an illiterate jury have, in spite of medical evidence, succeeded in doing the right thing; but it is also a case which shows how very frequently moral insanity is connected with intellectual delusion. Indeed, we are convinced that many observers have not—in their anxiety to prove the fact of a kind of insanity which exists independently of any prominent intellectual symptoms—been sufficiently careful to look for signs of the existence of that which they did not wish to see. Many people like Nelson—when he was told that there was a signal from the Admiral’s ship commanding his return—put the telescope to their blind eye, and say, “I cannot see anything.” So Dr. Ray^a quotes the case of the Earl Ferrers, who was executed in 1760^b for the murder of his steward, in illustration

^a Medical Jurisprudence of Insanity, p. 119.

^b See Hargrave’s State Trials, Vol. x., p. 478.

of what he regards as moral insanity. Dr. Ray does not, in the description he gives of the condition of the accused—in which he asserts that the disease was in a more advanced condition—state that it was proved that his lordship was occasionally insane, and incapable, from his insanity, of knowing what he did, and of judging of the consequences of his actions. He laboured under the delusion that his relations and friends had formed a conspiracy against him, and he regarded Johnson, his victim, as an accomplice. His conduct was of such a character as to convince those who knew him of his insanity. That the verdict of guilty may have been erroneous, and that the sentence and execution may have been inexpedient, is true, but that the accused laboured under moral mania seems to us false. In another place we point out the relation of those afflicted with intellectual mania to the State—here we would, while we praise the caution of our courts of law in hesitating to recognize moral insanity, and point out that, from the rarity of cases in which this disease is unaccompanied by very prominent intellectual symptoms, very little injustice has been done in consequence of the law's unwillingness to recognize this kind of insanity—we would censure the dogged persistence of lawyers who will not, even in the present state of medical psychology, and with the amount of evidence which has been accumulated, admit that there can, or ought to be, a recognition of such a form of disease by our criminal law.

ART. XIII.—*On Acute Inversion of the Uterus.* By THOMAS MORE MADDEN, M.R.I.A.; M.R.C.S., Engl.; L.K.&Q.C.P.I.: L.F.P. & S., Glasg.; Senior Assistant-Physician to the Dublin Lying-in Hospital (Rotunda); Corresponding Member of the Gynæcological Society of Boston, U.S., &c., &c.

INVERSION of the uterus is one of the rarest and most dangerous complications of parturition. The rarity of this accident is well illustrated by the Statistical Reports of the Master of the Dublin Lying-in Hospital (Rotunda); and as these reports are more familiar to myself, as one of the medical officers of this Institution, than any others, I shall commence by referring to them, not only on that account, but still more so from the fact that these statistics afford, as Dr. Mathews Duncan, of Edinburgh, has shown, the largest, and, perhaps it might be added, the most accurate mass of information on all obstetric subjects in existence.

Drs. M'Clintock and Hardy, in their *Practical Observations on Midwifery*, state that—"No example of acute *inversio uteri* has ever fallen under our notice; and the accumulated experience of Drs. Clarke, Labatt, Collins, Kennedy, and Johnson, in this hospital, does not furnish a single instance of the occurrence of this accident, though the number of women delivered during their united masterships amounts to upwards of seventy-one thousand."

During Dr. Shekelton's mastership, when 13,748 deliveries took place in the Rotunda Hospital, inversion of the uterus occurred in one case. Dr. M'Clintock, in his mastership, when 9,181 cases came under his care, met with no instance of acute inversion; nor did Dr. Denham, in his 9,867 cases; and up to the end of the year 1868, when 190,883 women had been delivered in the hospital since its foundation in 1745, only one case of acute inversion was observed, nor has one occurred since then.

Therefore, as I think that every medical practitioner who meets with any rare or interesting case, should place the facts he observes on record, I shall, before entering on the consideration of this subject at large, first narrate the particulars of a case of inversion of the uterus which recently came under my notice.

May 19th, 1870.—Whilst lecturing at the hospital, I was sent for at a quarter to six o'clock, p.m., by Dr. Torney, of Ellis'-quay, to a labour case. I drove there directly, and on arriving at the patient's residence I found Dr. Torney in attendance, and the patient in a state of collapse, pulseless, icy cold, blanched, respiration sighing, and exhibiting all the symptoms which generally attend cases of great hemorrhage. That this had taken place before my arrival, was evinced by the saturated condition of her bed, although the midwife denied that such had been the case.

On examination I found the uterus completely inverted, protruding between her legs, with the placenta firmly adherent to the fundus, from which a considerable draining of blood was still going on.

The history of the case was that the patient, M. L., aged eighteen, had been delivered of a living child, at full time, at a quarter past four, p.m. She had had a very quick and easy labour, which commenced at eleven, a.m., that morning, although the membranes were ruptured immediately before the first true pain occurred. It was her third pregnancy, as she had been married when only fourteen and a half years old. After the birth of the child the midwife who attended her, as, unfortunately as it turned out, no accoucheur

was engaged, informed me that there was considerable hemorrhage during the third stage. The placenta being retained she endeavoured to press it off, but not succeeding in effecting this, she sent for Dr. Swan, of Steevens's Hospital, but as he was out at the time, she stated that she introduced two fingers into the os to ascertain if the placenta was adherent or not, directing another woman in the meanwhile to make firm pressure over the fundus uteri. She denied pulling the funis, or even leaving it on the stretch at this moment, when suddenly, as she alleged, from the pressure of the woman who was pressing down the fundus, the womb became completely inverted and was extruded from the vulva. She now sent for Dr. Torney, who came at once, and on his arrival, finding the woman pulseless and moribund, and considering the serious nature of the case, sent up to the hospital for assistance, and in the meantime very judiciously administered stimulants, and applied a sinapism over the heart.

When I arrived, the woman was in the condition just described, and accordingly, having first administered as large a quantity of brandy and ammonia as I could get her to swallow, I at once proceeded to peel off the placenta which was morbidly adherent to a great part of the fundus. She was lying in a pool of blood when I examined her, and the hemorrhage was going on to a considerable extent before and during the operation, but was not at all increased by it. On the contrary it stopped almost immediately after it. I then returned the uterus, still inverted, completely within the vagina; the part that extruded last being the first returned, and applying pressure steadily to the fundus, with great difficulty succeeded in pushing it through the inverted cervix, there being a persistent convexity of the fundus uteri which it took some time to overcome. I now had the satisfaction of feeling the uterus spring out back into the pelvis before my hand and resume its normal condition. Dr. Torney also introduced his hand and found the parts in their natural situation.

We gave twenty drops of Battley's liquor opii sedativus with aromatic spirits of ammonia. She was still cold as death, jacitating, colourless, and pulseless.

I should now, without hesitation, have at once resorted to transfusion, and indeed was quite prepared to do so, as the case was, in my opinion, one in which this operation would have afforded the best chance of saving the patient's life. But unfortunately the circumstances of the case were such as to render it impossible to do

so, as there was no one at hand to furnish the vital fluid to be injected, and hence I was obliged, most reluctantly, to abandon the idea of transfusion. Transfusion then being out of the question, we applied fresh mustard sinapisms over the heart and to the calves of the legs, and hot jars were put to the feet. There was hardly any draining from the vulva; the uterus contracted firmly; and we bound her up and applied a large compress over it. The brandy and ammonia was administered by the tea-spoonful every couple of minutes, her head was lowered, and finding the vital powers failing still more and more, I again gave thirty drops of Battley in brandy and ammonia, in all fifty drops of Battley within half an hour's time. Spite of all our efforts, however, she sank rapidly and died before seven o'clock, p.m.

Dr. Ramsbotham narrates a case very like mine, both in the alleged cause, symptoms, and result. "I was called," he says, "on April 4th, 1866, to a case of inversion which occurred under the hands of one of the most careful of our midwives, in the Royal Maternity Charity, in which I was assured that not the least effort had been made to extract the placenta. The patient was dying when I arrived, about an hour after the occurrence, although there had been very little hemorrhage. She seemed to sink from the shock communicated to the system in consequence of the accident. The uterus was very placid; I restored it without difficulty to its natural position; but the woman expired before I left the house."^a

A case of this accident, which has fortunately been the only one that has occurred in this hospital from its foundation to the present date, is narrated in Drs. Johnston and Sinclair's *Practical Midwifery*, and was brought before the Dublin Obstetrical Society by Dr. Johnston in the Session 1853-54. In some of its features, particularly in the assigned cause, this case resembles that which I have just spoken of. The patient was nineteen years of age, and was delivered of her first child after an easy labour of six hours' duration. "The gentleman on duty, after having tied and separated the funis, had maintained the contraction of the uterus with the hand above the fundus—in accordance with the usual practice of the hospital—for a quarter of an hour, when finding a tendency to 'draining,' he increased his pressure, but, he asserted, not nearly to the extent it had been on frequent occasions found necessary to employ in order to assist in the expulsion of the placenta, or to restrain hemorrhage.

^a Principles of Obstetric Medicine and Surgery. By Francis H. Ramsbotham, M.D., p. 429. Third Edition. 1868.

The uterus was felt suddenly to yield and recede from his grasp; he immediately saw it expelled from the vagina; an inverted mass with the placenta still attached to its surface. The assistant on duty having been sent for, on arrival found the woman pallid, exceedingly anxious, complaining of considerable pain, with the pulse scarcely distinguishable. Examination proved the uterus to be inverted, with the placenta attached to its fundus; the funis was of ordinary length, and *there was no hemorrhage.*" Dr Johnston at once effected reduction, and the patient recovered perfectly in a very short time.

Inversion of the uterus is met with in either the acute or chronic state, and on the present occasion I shall limit myself to the consideration of the former. Dr. Churchill^a has proposed to substitute the terms reducible and irreducible for acute and chronic inversion; but, inasmuch as chronic inversion may be reducible whilst acute inversion is occasionally irreducible, I think that for our purpose the old names may be more conveniently retained.

By acute inversion then we mean to describe the form of this accident, which is of recent occurrence, and, with Dr. Barnes, I would restrict the period within which the case remains one of acute inversion, so long only as the physiological changes which take place in the uterus after, and in consequence of parturition, are going on.

Inversion of the uterus occurs in three different forms, or, more properly speaking, degrees of inversion. In the first there is merely some depression of that portion of the fundus to which the placenta was attached, which forms a cup-shaped projection into the cavity of the uterus. In the second variety of this accident the amount of inversion is much greater, so that the depression of the fundus is converted into a regular intus-susception of the upper portion of the uterus, which is forced down towards the os; and, lastly, there remains the third variety or degree of inversion of the uterus, in which the displacement is complete; the fundus and body of the uterus passing through the cervix, which with the os is, in some cases, also turned inside out, and protruding from the vulva.

The causes of inversion of the uterus may be either connected with parturition, or result from morbid growths within the uterus. The former class of causes being more frequent than the latter, in

^a Manual of Midwifery. Fifth Edition. By Fleetwood Churchill, M.D., p. 569. Dublin, 1866.

the proportion of seven to one. In the great majority of cases the cause of inversion of the uterus is unskilful treatment of the third stage of labour, the womb being inverted by forcible traction on the funis, or by undue pressure over the fundus to hasten the expulsion of the placenta. And hence it is that in this hospital, where great caution is observed in every stage of parturition, this lamentable accident has occurred but once in the course of one hundred and twenty-five years.

Inversion of the uterus may, however, occur quite irrespectively of any malpractice, and this fact, which it is most important to bear in mind, is attested by William Hunter, Baudelocque, Crosse, Tyler Smith, Cowan, Barnes, and M'Clintock.

There can, I think, be very little doubt that the most frequent cause of inversion of the uterus is the employment of undue force in drawing down the funis to remove the placenta. But this is not the sole cause, for cases are recorded in which this accident was produced by shortness of the cord,^a or by the pelvis being abnormally large, by delivery taking place when the patient was in the erect position, by inertia of the uterus, by precipitate labour, or by the patient making extraordinary voluntary or involuntary straining, expulsive efforts to force off the placenta. Or, as was alleged in the case just narrated, by excessive pressure being exerted by the attendant on the fundus uteri for the same purpose. The possibility of spontaneous inversion of the uterus taking place has been strenuously denied by some writers; but too many cases to the contrary have been recorded to admit of any doubt that this accident may occur without any tangible cause. These cases, however, are but exceptions to the general rule, and in the majority of instances of inversion of the uterus, it has been the result of improper traction on the funis when the placenta was still adherent to the uterus.

Spontaneous inversion of the uterus was first described by Hunter, who says—"The contained or inverted part becomes an adventitious or extraneous body to the containing, and it continues its action to get rid of the inverted part, similar to an intus-susception of an intestine."^b Dr. Burnes supported the same view as Hunter, and held that in every instance of so-called spontaneous inversion of the uterus, a partial displacement or depression of the fundus had

^a Dr. Denman's *Introduction to Midwifery*. Seventh Edition, p. 423.

^b See "*Pathological Catalogue and Preparations*," Royal College of Surgeons of England.

previously existed, and the change to complete inversion, the gradual and natural result of this.

The occurrence of spontaneous inversion of the uterus is, as Dr. Barnes points out, intimately connected with inertia of that portion of the fundus to which the placenta was attached; this portion of the uterus being thicker than the rest of the walls of the organ, naturally projects inwards, and this projection inwards, or depression when thus produced by inertia, is described by Dr. Barnes as the first degree of inversion.^a

As to the diagnosis of a case of this kind there can be no difficulty whatever in recognizing the nature of the accident which has occurred, when the inverted uterus is extruded from the vulva with the placenta still attached to the fundus. The diagnosis is by no means so simple, however, if the accident has occurred after the expulsion of the placenta; or if the placenta has been detached before the arrival of the accoucheur. The symptoms of inversion of the uterus are always of a very grave and alarming character, being of course more marked in the complete than in the partial varieties. Amongst the most constant and most prominent are those which mark the amount of shock which the nervous system sustained by it. At the moment of the occurrence of the accident, the patient generally experiences sudden and intense bearing down pain in the womb. This is almost invariably immediately followed by a feeling of faintness and exhaustion, and the setting in of sudden collapse; the pulse becomes rapid, weak, and intermittent, the skin cold and clammy, vomiting, or, at least, nausea is present; the respiration is hurried and sighing; she tosses about, moaning continually, and soon becomes unconscious. These symptoms are of course aggravated by the profuse hemorrhage which generally attends inversion; but they exist also in cases in which little or no hemorrhage has occurred.

On examining the parts, if the inversion be complete, the nature of the case is self-evident, but if it be only partial, or, in the second degree, we must make a vaginal examination, when a tumour will be found in the pelvis, of a globular form, dipping down into a *cul-de-sac* all around it, about an inch or more in depth, within the os uteri. If the case be merely one of simple depression of the fundus, it will not be possible to reach the inverted part without introducing the hand within the uterus. But here we must, as in all these cases, examine with the hand above the pubis, when, if the

^a Lectures on Obstetric Operations, by Robert Barnes, M.D., p. 456. London, 1870.

case be one of complete inversion, or even of extreme introversion, we shall not find the hard contracted uterine tumour in its natural position; and if the case be one of depression, we shall be able to trace the outline of the depressed fundus through the abdominal parieties.

Though it would be almost impossible that complete inversion could be overlooked, yet it is by no means impossible that partial inversion might not be at once recognized. Hence, if a patient after delivery complains of sudden intense bearing down pain, or evinces symptoms of collapse and shock, not to be accounted for by any form of hemorrhage, the practitioner should bear in mind the possibility of the case being one of *inversio uteri*, and at once institute a vaginal and abdominal examination, and if he does not find the globular fundus uteri in its normal position, and if he discovers any tumour projecting into the vagina, he should consider and treat the case as one of partial inversion.

It has been noticed by several authorities that it frequently happens that complete inversion of the uterus is accompanied by far less hemorrhage than is generally the case when the inversion is only partial.

In the treatment of inversion of the uterus, success or failure is mainly determined by the promptitude of the accoucheur in effecting the reduction of the displaced organ. For if this be postponed sufficiently long to allow the uterus to contract completely, replacement will be impossible, and the patient will either perish at once from hemorrhage and shock, or survive the victim of the most distressing uterine suffering.

Denman says that he found it impossible to reduce an inverted uterus after a lapse of four hours from the occurrence of the accident.

Cases of spontaneous reduction of an inverted uterus are related by several writers; but these cases are so exceeding rare as to be of no value whatever in forming the prognosis of a case of this kind, or in militating against the imperative necessity for at once endeavouring to replace the womb in its natural situation. Baudelocque narrates a case where, after a lapse of eight years, an inverted uterus was spontaneously restored to its normal condition. And this has been accounted for by Dailliez, on the supposition that the tubes pulled up the inverted organ.

It seems almost unnecessary to add that in attempting to return the inverted uterus to its natural situation, it is essential, while

replacing it, to bear in mind the direction of the axes of the pelvis, and to press the uterus at first upwards and backwards into the hollow of the sacrum, and then upwards and forwards through the brim, in such a direction laterally as to avoid the promontory of the sacrum, which Dr. Barnes and Dr. Skinner, of Liverpool, both agree in thinking may prove an important obstacle to the reduction, if not avoided by this lateral movement.

Dr. M'Clintock^a endorses by the weight of his high authority and great experience, the suggestion of the late Dr. Montgomery, namely, to replace that portion of the inverted uterus first which came down last; and he has most clearly shown the necessity for attending to this point. For if any attempt be made to force the fundus through the cervix whilst the uterus is still in a state of procedentia the result will be to produce double inversion, and, therefore, increased difficulty in pushing back double the thickness of the uterine substance through the constricting cervix.

The difficulty of replacing a completely inverted uterus, results from the constriction of the inverted part by the cervix; every hour increasing the tumefaction of the protruded organ, and the pressure of the neck of the uterus through which it must pass, until reduction through the undilated cervix soon becomes impossible. The case is now very closely analogous to one of the strangulated hernia.

As on all subjects connected with the practice of obstetric surgery, the question of removing or not removing the placenta from an inverted uterus, before attempting the reduction of the displaced organ, is one on which the opinion of obstetricians are almost equally divided. Following Denman,^b most English writers advise that the uterus should be replaced with the placenta still adherent; this practice is laid down by Blundell,^c Davis,^d Ramsbotham,^e Burns,^f and Newnham.^g

Denman and Blundell, however, both agree that if the placenta be separated to a considerable extent, it should be removed before the uterus is replaced, and that, if not, it should not be interfered with until the organ be returned to its natural position.

^a M'Clintock—*Clinical Memoirs on Diseases of Women*, p. 76. Dublin, 1863.

^b Denman—*Introduction to Midwifery*, cap. 15, p. 12.

^c Blundell—*Obstetric Medicine*, p. 693.

^d Davis, p. 1088.

^e Ramsbotham—*Obstetric Medicine and Surgery*, p. 430.

^f Burns—*Principles of Midwifery*, p. 501.

^g Newnham on *Inversion of the Uterus*, par. 1309.

Dr. Barnes recommends the uterus to be replaced with the placenta still adherent, if the accident be recognized immediately after its occurrence. "But if this favourable moment, it will be better," he says, "to detach the placenta first."^a

The reasons assigned by Dr. Ramsbotham and Mr. Newnham for not removing the placenta prior to replacing the uterus, are that "either the woman must lose a very great quantity of blood from the patulous orifices of the exposed vessels, or if such a degree of contraction took place as to stop the hemorrhage, that the very shrinking of the uterine parietes would preclude the possibility of restoring it to its natural state." Mr. Newnham goes still further than Dr. Ramsbotham, and says, "Besides returning the placenta while it remains attached to the uterus, and its subsequent judicious treatment as a case of retained placenta, will have a good effect in bringing on that regular and natural uterine contraction which is the hope of the practitioner, and the safety of the patient."

Amongst Dublin obstetricians the weight of authority is undoubtedly in favour of removing the placenta, if adherent, before attempting to reduce the inverted uterus. Dr. George Johnston, by his own example and precept, has strongly supported this practice.

Dr. Churchill says:—"I have no doubt, therefore, that as removing the placenta would facilitate the reduction of the inversion by reducing its volume, that the proper method in general would be to peel it off before attempting to restore the organ."

The mode of effecting the reduction of the displaced uterus, though the subject of much controversy in some of the standard works on the question, is when freed from the unnecessary circumlocution in which the subject has, I think, been involved, is in reality simple enough. The placenta, if adherent, having been first gently, but as quickly as possible, peeled off, the displaced uterus is to be grasped in the operator's right hand, and pushed firmly, in globo, into the vagina, the cervix being first returned. No attempt should of course be made to re-invert the uterus until fairly within the vagina, as otherwise a double inversion would be the result. But, when the uterus has been passed as high up within the vagina as possible, steady pressure should be made with the pulp of the three fingers within the vagina on the fundus, which should thus be pressed through the cervix, great care being taken that the

^a Barnes' Lectures on Obstetric Operations, p. 459. London, 1870.

pressure should not exceed the resisting power of the uterine wall, or else a fatal laceration of the fundus might be very easily produced; in this way the fundus should be insinuated back through the cervix, when pressure with the knuckles may be substituted for the fingers, and continued until, as generally happens, the uterus will be found to spring out of itself upwards and forwards, "like a bottle of india-rubber when turned inside out," as has been aptly observed, and the womb resumes its normal condition. The operator's hand should now be left in the uterus until, as in my case, the organ is found to contract strongly on it, when it may be withdrawn, and the case treated on those general principles applicable to all cases in which a great shock and much hemorrhage has been sustained by a parturient woman.

If the inverted uterus cannot be replaced after all our efforts have been pushed as far as is possible compatibly with the safety of the patient, the uterus should be, at least, pushed up within the vagina; hemorrhage should be restrained by astringent applications; inflammation should be met by appropriate treatment; the patient's strength supported; the state of the bladder carefully attended to; and all pressure taken off the part as far as possible by properly adjusted bandages or pessaries, as the case may be. I need not consider this subject further, as the case is one now one of chronic inversion of the uterus.

ART. XIV.—*Hydrate of Chloral and Nitrous Oxide Gas as Anesthetics.* By CHARLES KIDD, M.D.; Member of the College of Surgeons, England; Associate Member Surgical Society of Ireland; late Physician to Westminster General Dispensary, &c.

I WOULD wish to supplement some former observations on anesthetics by a few remarks of a practical kind on a further three years' experience of nitrous oxide gas, chloroform, chloral hydrate, and one or two other agents of this class. The nitrous oxide begins to be used now mixed with atmospheric air, which allows it to be adapted to long operations, and much improvement has taken place in ophthalmic practice in "quick anesthesia," especially at Guy's Hospital.

The present year will be sadly memorable, for establishing in field hospitals, during a terrible and prolonged war, the great value and importance of two new anesthetic or narcotizing agents, the

hydrate of chloral and nitrous oxide gas; of the latter, I took an opportunity of specifying its peculiar physiological advantages in short operations, a little while ago, especially in removal of small tumours, dentistry, &c. Military surgery, we need scarcely remark, differs in little from ordinary civil surgery; primary operations are now indeed facilitated, "shock" lessened, excision of joints or extraction of bullets or fragments of shell, made easier, by those benign and quick-acting agents, nitrous oxide, for which it is indebted to dentistry, and hydrate of chloral, so advantageous either with or without chloroform.

The chief reasons for dwelling on this "gas" are, perhaps, two—viz., its greater safety; and now the chance of its being more used in practice, as it has been fluidified. There is, indeed, now an excellent variety of pain-removers or anesthetics; the wounded, notwithstanding conical bullets, needle-guns, and *mitrailleuses*, have reason to bless the happy discovery of chloroform, nitrous oxide, and chloral. Something like £250 was given in donations, in London, recently, by the public, specially for their purchase, to be sent to the seat of war; at a time, perhaps, when also there existed considerable though idle prejudice to chloroform, together with the old ideas of stuffing all incised sabre wounds with lint and cotton! Short operations are best done with the nitrous oxide, as I had an opportunity of explaining now some two years since, when it was by theory very generally condemned as the most deadly of all anesthetics on animals. That supposition has not been realized in man. Reference was then made to the influence of this gas on the muscles, so different to chloroform, the peculiar "twitching" and stimulating action, and the astonishing rapidity with which the effect of the gas wears off. The exact cause of death from chloroform, or the causes of at least three forms of accident, as Mr. Erichsen would say, are yet subjects of debate. These causes are probably to be eliminated by what Mr. J. Stuart Mill would term the "method of agreements" and differences. The deaths are typical of one or other of three kinds; agreeing now with the phenomena of apnea, now again agreeing with shock or asphyxia, &c. The statistical or chemical school says there will be always one death in some even factor of hundreds of operations, like shipwrecks, and it is as little use to strive to prevent one as the other!

The deaths to me appear rather special, well marked, or typical, from chloroform, and in many instances to be prevented by quick

and skilful use of electricity, artificial respiration, turning the patient over on his side, so as to relieve the engorging heart, &c.

In estimating the alleged greater danger of chloroform, we should not overlook the inconceivably greater number of times it is used as compared with the ether or methylene compounds all over Europe; the number also of deaths that may be simple coincidents, which before the era of chloroform were ascribed to "shock." It is, however, a vast relief to the minds of many surgeons that in practice, whatever theory may say to the contrary, deaths are almost unknown from even the deepest narcotism of nitrous oxide, and that most probably the "cardiac syncope" idea of Clover and others is entirely erroneous as to chloroform.

These general formulæ of pure chemistry, as to voltaic narcotism, ethyl-methylic ether as superior to the chlorine "family," the invariably deadly effects of nitrous oxide gas on animals, &c., require, in fact, the correcting influence of large hospital experience. Military surgery, indeed, as just hinted, aided by the efforts of the public, will have solved the difficulty, in conjunction with the ever increasing demand for the "gas," in the condensed form. It may be true, that nitrous oxide gas, by theory, is the most deadly of all anesthetics, or that blood coagulates only by escape of ammonia, as theory supposes; but the chief physician at Guy's tells me, though their school originated the latter fancy, they do not now believe it in actual practice. Amputations under the "gas" are now numerous; in one, the patient was nineteen minutes under its influence. Its immense safety is its chief recommendation. It stimulates in "shock," so that we cannot be led solely by formulæ or theory. Its action to the patient is pleasant, and there is no vomiting (the great trouble of unskilful chloroform administration).

Next let us speak of the chloral hydrate, large quantities of which were eagerly asked for in the war. We hear now of large amputations under a draught containing this new medicine, thus suddenly realizing the magic in the potion in *Romeo and Juliet*, the borrowed likeness of shrunk death:—

"That cold, sad, drowsy humour which shall seize each vital spirit."

A very extended "Report" on the chloral hydrate was given not long ago, with remarks on its effectiveness as a sedative and sleep-producer in sundry nervous affections. All that report I would wish to endorse or further exemplify. Since then these various uses of this agent have been very fully corroborated,

especially in the experiences of the Edinburgh school. It is now found that the chloral hydrate furnishes us with a safe and satisfactory means of rendering operations in midwifery more easy, as well as taking away pain in the first stage of childbirth itself, producing that wished-for calm and invigorating sleep usually observed as the first effect of administration of chloroform in that state. This sleep, indeed, in midwifery practice, always strikes me as half the battle in a case, as thereby the labour is made more active and safe. Additional experience, year by year, does not diminish, if it does not increase, the anxiety of the public as to the much dreaded chloroform; especially (as we have so often contended, though so difficult to explain,) in short, slight, or trivial operations—death, sudden, and when least expected—the nitrous oxide gas, now liquified, on the contrary, as safer and pleasanter in administration than chloroform, has had, therefore—as I explained two years ago—an easy triumph, especially in the trouble of teeth extraction and dentistry generally, while for bullet extraction during the war, Dr. Thudicum has recommended it, but this chloral hydrate may supersede both.

Actual facts, in surgical practice, are often in curious contradiction to what theory in the laboratory, unsupported, would suggest. I have for several years dwelt on a fact that Dr. Richardson has recently learned, that adult persons in rude health, nervous or alarmed, about to undergo a surgical operation, are those most difficult to be anesthetized. It is Millar's "law of tolerance," and nothing can be more exact than the original idea. The accident does not occur in shock, exhaustion, or deep coma. Richardson likewise assumes, from experiments on an already dead rabbit, that electricity will not revive it, as electricity by theory exhausts the remaining physical or nervous force, &c.; but practically, in hospitals, electricity is now our one great hope, but is seldom applied correctly. Electricity, in fact, sets up the best form of artificial respiration in apnea from chloroform, or in drowning cases.

A few technical or empirical facts may be here worthy of being noted, or perhaps repeated.

The plan of alternating ether with chloroform is a good one; the ether in a separate inhaler, which restores a flagging pulse better than anything else I am acquainted with; 20 or 30 drops of sal-volatile in a little water immediately before operation, is also better than brandy. An aloetic purgative the night before an operation helps to prevent vomiting. Bichât tells us that the carbonic acid

of venous blood is the proper stimulus of the lung; so that it is to be feared oxygen gas as proposed for inhalation will not prevent death from chloroform; transfusion of warm water with ammonia would be better worth trial.

The explosive, or rather expansive, power of the condensed nitrous oxide—something at one time compared to a soda water bottle, then to a pocket pistol—has been partially got over by further condensation of the gas into a liquid.

The respiration is, perhaps, more important to watch than the pulse. They are, shall we say, rather opposite swings of the pendulum that regulates the heart circulation. This is applicable to the administration of the gas as well as the administration of ether or of chloroform. The nitrous oxide must be used at first undiluted with air to secure insensibility. But it is advisable in prolonging the action of the gas that it be mixed with air. An immense trade is now carried on in this gas, and some five thousand gallons of it have been despatched to the seat of war, in Paris!

It is worth remembering that very complete anesthesia may be produced by injecting the venous blood of the jugular into the carotid artery of the same animal; nay, that anesthesia is sometimes possible without complete unconsciousness.

Cardiac syncope is caused by stimulation of the inferior branches of the vagus, as mentioned several years ago, but it has recently been put forward as new by the chemical school, with the further explanation that this stimulation results from the venous condition of blood. Brown-Sequard and Scheinessan have discussed the point. But experiments in the laboratory, or on animals, where the latter are simply suffocated, it cannot be too often repeated, are not the same as cautious experience with anesthetics on man, where every possible care is taken that accident or smothering shall not take place; hence the errors of the chemical school. We have practically very little to do in hospitals either with "syncopal apnea," or suffocation of animals in 20 per cent. of chloroform, as exhibited in popular lectures, with suffocation by prolonged nitrous oxide gas; and surely, if cardiac syncope is caused by the venous condition of the blood, the venous condition of the blood under this "gas" is most alarming, and yet without cardiac syncope.

The most lamentable results occasionally arise at coroners' inquests, or in other legal proceedings, as I have elsewhere explained, from this popularizing of the idea, that chloroform, as at lectures, is a most deadly agent under every circumstance, whereas, with care and average skill, it is as safe as any other anesthetic.

Military surgeons need not be afraid of their deadly experiments, or syncopal apnea, or electricity in impending death. All goes right while the respiration, as just said, is good; but now fifteen grain doses of the chloral hydrate, repeated two or three times, will produce a state like the sleep of chloroform. Three cases of tetanus, after the battle of Sedan, were wonderfully relieved by the chloral hydrate. Military surgery is thus improved.

Surgeons in war should have correct views on such chloroform accidents. A now dead superstition as to large balloon apparatuses had impeded progress, and been the cause of several deaths and actual imprisonment of a surgeon in China because death had happened!

In a word, sudden accidents from a cautious administration of chloroform in small half-dram doses, where alarm in the patient's mind, exceptional depression, or idiosyncrasy, are at work in causing syncope, are most probably altogether unlike the state of the system in animals, slowly suffocated by large doses. This syncopal apnea which alarms popular audiences, as well as the always deadly action of the nitrous oxide gas, at least on the lower animals, are also not consistent with plain hospital experience. And so is it, as far as I have seen now in at least a dozen cases of impending death from apnea, where electricity has restored life, we cannot say exactly when the physical or nervous force is in danger of further exhaustion, though theory at popular lectures may do so. We cannot say mere physical force is nervous force and nothing else, as the chemical school would do.

The controversy, indeed, widens till, as old Burton says, somewhere to cut down one head of this hydra, is to give occasion for another head to spring up. Military surgery, however, as said, is improved *malgré* the denunciations against the nitrous oxide and chlorine "family," or popular solemn balloons, or fright of heart disease. Dr. Chapman very early established the nature of death from chloroform as attended with dilated right heart. Lallemand and Perin have superabundantly demonstrated it. Dr. Richardson has stated the idea again as new, but with a supposition that in such accidents the patients ought to be sickly, with varicose veins and other indications of heart disease, or sluggish venous system, and yet we know as a rule they are persons in rude health, generally for trivial operations; in the administration of the nitrous oxide, on the other hand, there is intense congestion of the venous system, but no danger, so that we are met with

contradictions or anomalies at every side; a livid, cold, death-like appearance, as I described before, with venous congestion, suffocation, and impending death, but still not the least danger; and so is it, one sometimes thinks of chloroform as one has seen it, and in over ten thousand administrations without one death, yet in "sensation" lectures with the electric light and camera, skin diseases in London, and death always under chloroform from heart disease, and this meaning malpraxis so dear to coroner's juries, theory opposed to every day observation, the instinct of the public right to subscribe nevertheless, disbelieving in fatalism and statistics, painless dentistry only by voltaic narcotism or ether spray, all these things keep up a good deal of popular error that it is well to understand.

Yet thus we go on widening the controversy as to these several agents, popular errors not diminished, theory often opposed to plainer practice, the nitrous oxide condemned but adopted by the public.

The changes in the blood during the action of these medicines is also suggestive and peculiar. Formiate of soda, according to Liebrich, one of the results of the chloral administration, while with sulphuric ether the blood corpuscles are altered; less so under chloroform. But during the use of the nitrous oxide the presence of this gas cannot be detected, even by spectroscopic help or other test, so that it (nitrous oxide) seems to induce a form of asphyxia by mere absence of air; these several changes of the blood taken in connexion with divers experiments with ammonia to re-act on the deadening pyemic poison of puerperal fever, as explained to me by my excellent friend Dr. Tyler Smith, open up a new vista in practice of great hope and promise. Theory has already condemned Dr. Tyler Smith also, but further practice does not do so, the theory that condemned the nitrous oxide gas! Laboratory theory or speculation, as said so often, has condemned the nitrous oxide as inferior to some methylic ether and methylene compounds; Voltaic narcotism, or ether spray in dentistry; several alarming deaths have occurred from bichloride of methylene, but none perhaps that can be fairly ascribed to the nitrogen protoxide, though exhibited now in some hundreds of thousands of cases. The methylic ether also having proved dangerous, if not useless, in general practice, so that we cannot trust too much to laboratory ideas built up on the molecular composition of methylene or chloroform. Nor am I sure there is any solid foundation in this

condemnation of the "chlorine family" of anesthetics that would deny all usefulness to the chloral hydrate; a new school with a dogma of infallibility is, however, now in power in London, where the patient's mental or nervous state before taking an anesthetic counts for nothing, and theory sets aside clinical teaching, with its slowly arrived at but invaluable facts, for superstitions worthy of Apollo or Paracelsus, as to three and a decimal per cent. of vapour in a balloon, with patent stop-cocks, as above, and only safe, else (in China) it proves manslaughter if death occurs. No matter, numerous deaths already with the impossible cocks and valves, so like the invariable danger of the nitrous oxide condensed, as just said, by abstract theory, but adopted by the public; or like the chlorine hydrate of the deposed chlorine family, or dynasty, the controversy widens, but military surgery and the public have been benefited *nos mentimur omnia*. "We are all deceived at times," says Plautus. The wounded and sick now, as in the Crimea, were a month without chloroform—the public at first frightened—we only strive to smile away these fears.

ART. XV.—*Clinical Notes from Kilkenny County Infirmary.* By ZACH. JOHNSON, A.M., T.C.D., F.R.C.S.; Licentiate of the College of Physicians, &c., &c.

WHATEVER grounds may exist, real or apparent, for impeaching the exactitude of medical science, it must be admitted that when we test our conclusions by the inexorable laws of figures, we cannot deviate very much from the precision of mathematical deduction; and thus, when, in that branch of the divine art of healing to which the term surgery has been appropriated, we decide the character of great operations or formidable innovations by a strict reference to faithful statistics, we establish our conclusions on a basis as certain and unerring as any which our fellow-workers in the various other fields of science and philosophy can boast as the foundation of their discoveries and dogmata.

This reference of dubious or debateable questions to the impartial testimony of statistics, is perhaps one of the most remarkable and important characteristics of modern medical science, and one which must go far to relieve our noble profession from the opprobrium of inexactitude. And as every contribution to the statistics of any vital or important question must be of value not alone to those

engaged in the prosecution of our art, but to humanity at large, so the suppression of instructive fact or decisive evidence would seem to amount to a dereliction of duty and an offence against the common weal

Among the startling innovations of modern surgery, none perhaps are of greater importance than that class of operations known as the "excision of diseased joints." The practical surgeon alone can have any idea of the many cases in which, by the adoption of this proceeding, a useful limb is now preserved, where, only some 25 or 30 years ago, amputation would have been unhesitatingly pronounced the only justifiable expedient, to rescue the sufferer from the exhausting and inevitably fatal ravages of agonizing disease. But great and unquestionable as have been the advantages resulting from the introduction of this important practice, the question remains to be decided, what class of cases are most suited for it, or, in other words, what is the average mortality, and what the ultimate advantage, resulting from the excision of the joints respectively; for we know that the excision of some are much more likely to be followed by a fatal result than that of others, while it is not certain that the member retained in some cases is as useful, always, as an artificial apparatus might have been; but these are questions which can only be decided by the statistics of surgery, derived from a comparison of the results of the greatest possible number of available cases.

These remarks are offered as some apology for now submitting the details of two successful cases, one of excision of the ankle-joint, and the other excision of the elbow, with a hope that they will prove of sufficient interest and importance to justify their publication. For although admitting fully the force of the adage that "one swallow does not make a summer," it must be allowed, on the other hand, that every unit is essential to the correctness of any calculation.

Excision of Ankle, eventuating in the Preservation of the Limb, with a Movable Joint, and only Slight Deformity.

The case about to be recorded is one of singular interest for many reasons. Its history is perhaps unique, and the attendant circumstances probably without parallel. Early in November, 1869, a respectable farmer, residing some eight miles from this city, sustained a compound fracture, with dislocation of the ankle, by a fall on a heap of stones from off a hayrick. The fibula was broken about an inch and a half above the outer maleolus; while the tibia

was partially driven through a large rent on the inner side. Another jagged rent on the outer side, several inches in length, laying bare the peronei tendons and deep-seated tissues, rendered the fracture of the fibula a compound one, and formed an opening into the joint on this side also. Thus the ankle-joint was laid open both on the outer and inner side.

The case was seen, not long after the occurrence of the accident, by Dr. Phelan of the Kilmoganny Dispensary, the torn and shattered parts properly adjusted, and subsequent displacement provided against by suitable appliances. Matters went on fairly, and without cause for any unusual anxiety of alarm, for some days, when Dr. Phelan received an urgent summons, and on his arrival found a state of things which at once caused equal astonishment and dismay.

The apparatus, which had been carefully adjusted by him, had been removed—all precautions against displacement had been abandoned. The spasmodic and refractory muscles of the leg had been permitted, unrestrained, to work their wicked will, and the consequence was that the tibia had been driven several inches through the wound over the inner malleolus, while the foot had been drawn upwards at an angle of some 45 degrees, on the outer side of the leg, presenting altogether a most formidable and unpromising spectacle.

This extraordinary state of things proved to have been caused by the interference of a functionary, known in provincial districts by the name, "Bone-setter." Such a person had been permitted to see the patient—as a matter of course he removed all apparatus and dressing, and finding the parts *in situ*, told the patient and his friends that everything which had been done was improper, that the limb did not require incumbrances, which were only causing inconvenience and doing injury, and accordingly, having removed everything which could prevent displacement and counteract irregular muscular action, he left the limb unsupported and unrestrained, to follow the dictates of those refractory forces which volition or spasm might arouse into operation.

The amount of injury which is inflicted on the credulous peasantry by these so-called bone-setters, is almost beyond belief. The utter ignorance of every principle of anatomy, physiology, or surgery, which characterizes these people, ought to work out a natural cure, and open the eyes of the credulous and misguided inhabitants of rural districts to their incompetency in the simplest case of real injury.

But the rural population blindly continue to repose a confidence in them above that which they accord the educated surgeon, no matter how glaring the errors of the first, or how high the reputation of the latter. Their blind confidence is, I believe, to be accounted for only by two facts. First, by the assumption of a happy designation, known only by the name, "bone-setters," ergo, they must be bone-setters; affording a practical answer to the query "what's in a name?" Secondly, in addition to making it a point to decry whatever has been done by the regular practitioner, they frequently *assume that there has been fracture*, when there has been *only contusion*, and of course are able to point triumphantly to the perfection of their work, in after times. Such imposition is of frequent occurrence, and seems to counterbalance the sad instances of mutilation and deformity which result from this ignorance and mismanagement. It is much to be deplored that no legislative measures have been devised to rescue the credulous from the consequences of such ignorance and malpractice. Provincial practitioners alone can have any conception of the extent to which such malpractices are carried, or of the deplorable and revolting consequences which are to be met with every day.

Being now called to see this man, in consultation with Dr. Phelan, the question at once arose, whether immediate amputation were not imperative? However, the patient and his friends were strenuously opposed to such a measure, and begged importunately that we should endeavour to save the foot at any hazard.

On reviewing the man's condition, we determined to make the attempt; but to restore the articulating extremity of the tibia to its natural position was out of the question. To overcome the contraction of the great and powerful muscles which had drawn the foot into its unnatural position would have been impossible, and were it not so, the condition of the extremity of the bone (which had now been protruded and exposed for many hours), and of the torn fragments of ligaments, cartilage and periosteum, which were already black and sphacelated, utterly prohibited their restoration to their natural position, even if practicable.

It was at once determined to saw off the sphacelated extremity of the tibia, and then to return the bone to its usual position, trusting to the efforts of nature to assist in establishing a false joint, or ankylosis.

Accordingly the lower end of the tibia was removed, and the bone replaced, so that the cut end rested on the articulating surface

of the astragalus. The ruptured ligaments and tendons were now arranged, as far as possible, *in situ*, and the lacerated skin and soft tissues drawn together as nearly as circumstances would permit, and the whole having been enveloped in suitable dressings, so as to exclude air, as far as practicable, side splints were adjusted, to prevent displacement and retain all in a proper position. A full opiate was next administered, which was again repeated at night.

It was arranged, with the consent of the patient and friends, that he should be conveyed carefully the following day, to the Kilkenny County Infirmary.

He bore the journey well. In a short time the torn and contused integuments on each side of the ankle sloughed away, as did also portions of ruptured ligaments and tendons, and there remained two chasms, each communicating with the joint, the broken fibula and tendons of the peronei lying exposed at the bottom of the outer one, synovia escaping freely from both, while the white articulating surface of the astragalus was readily seen from either side. At this time, by slight traction on the foot, light was visible through the original position of the joint.

The treatment adopted resolves itself into the constitutional and local. He was at once put on a very liberal diet—beef-tea, eggs, and, as soon as he could use them, chops, eight ounces of wine, and two ounces of whiskey, with milk and whey *ad lib.*, daily, and along with this, liberal doses of opium and quinine at frequent intervals, and a full opiate always at night.

The local dressings consisted almost altogether of lint steeped in carbolic acid lotion (one part acid to thirty parts water), and for some three weeks, every day the maleolar chasm was freely syringed with the carbolic lotion, which flowed freely in at either side and out at the other, over the astragalus, and all the other tissues. This was done in consequence of a profuse fetid discharge which accumulated daily about the end of the tibia and filled the chasm; and also because of the sphacelated condition of neighbouring tissues, which were only slowly separated and thrown off.

The smallest injury or inconvenience never appeared to arise from driving this lotion through the site of the articulation, while the articulating face of the astragalus was to be seen sound and unimpaired, until the process of granulation slowly filled up the articular chasm, and shut it out from view.

The result of this case is equally remarkable and gratifying. The patient remained under treatment several months. His

constitution bore up wonderfully; at no time did Hectic threaten with the exception of a dash of diarrhea at the end of the fourth week, which, however, yielded at once to a few doses of the ordinary chalk mixture. The opium, which was continued by day as well as by night, with the quinine, secured him rest and appetite; the liberal diet and wine, &c., supported his strength, while the carbolized dressings appeared to exercise most beneficial influences on the local injury, correcting the effects of sphacelus and decomposition, and stimulating healthy action to an extent which could scarcely have been anticipated at first.

For many weeks this man's fate may be said to have been trembling in the balance. It was long doubtful whether amputation would not have to be resorted to. His constitution, however, bore up wonderfully, and lent every assistance to the efforts of the surgeon. But even when the progress of his case permitted the indulgence of a hope that the limb might be preserved, the most sanguine spectator did not entertain the possibility of any result better than an ankylosed joint, and this was looked forward to as a triumph of conservative surgery.

The case, however, has eventuated in a manner which exceeds the most sanguine expectations. The sloughs were slowly thrown off, the purulent discharge grew less offensive and gradually less profuse, healthy granulations sprang up in every direction and filled up the chasm from below, while they closed in the gaping apertures at either side, until at last there remained only two fistulous openings, one at the site of each malleolus; as these closed in, abscesses formed from time to time in the neighbourhood, which proved to be only outlets for matter of deep-seated origin, and were easily relieved. The patient's general condition improved, *parri passu*, with the improvement in the injured parts, until it reached that of perfect health; but the most gratifying and unexpected result was, that a considerable amount of motion was preserved in the joint; nature appears to have formed an articulating surface on the divided extremity of the tibia, adapted to that of the astragalus, which remained healthy and unimpaired, notwithstanding its long exposure to atmospheric influences; and after a sojourn of some eight months in hospital, this man returned home in perfect health, and with a limb but little deformed. He is now attending to the business of his farm, and although not able yet to move about without the assistance of a stick, there seems little room to doubt that he will eventually be able to do so. Of course there must be some

shortening of the limb, but only such as can be compensated for by the addition of a few "lifts" of sole leather to the heel of the shoe or boot.

Although every description must fall far short of the actual condition of this man both before and after the resection of the tibia, yet the entire circumstances of the case were so peculiar, its progress so instructive, and its result so unexpected, that, although as a contribution to the statistics of articular excisions, it must count only as a unit, it would appear of sufficient importance to justify a publication of its history and details.

Excision of Diseased Elbow-joint; immediate relief from intense suffering and impending Dissolution—Preservation of a useful and efficient Hand.

If the preservation of the human foot be a desideratum, that of the human hand is vastly more important. The foot may be replaced by seemly and effective mechanism, emulating the perfection of nature both in appearance and utility; but no mechanism can replace the human hand—that wonderful organ whose uses and achievements have been so ably demonstrated by Bell.^a The hand human, without whose assistance man could not have emerged from the debased condition of the gorilla and the ape; by whose aid, guided by the intelligence of mind, he has achieved performances, which, compared with his physical and mechanical powers, might well be styled miraculous; by means of which stupendous structures have been raised, or works of microscopic minuteness accurately perfected; which has enabled him alike to master the elephant and the horse, and to subjugate the most subtle agencies; to make explosive vapors the tractable ministers of prescribed and definite force; and has constructed appliances which enable him to dive far into the inaccessible realms of infinity, whether he desire to scrutinize the transcendental arrangements of ultimate organization, or peer into the bewildering regions of illimitable space; has almost materialized thought, by means of the printing press and telegraph, and by harnessing it to the lightning, has actually accomplished an instrument which realizes the figurative conception of the poet, and "wafts a sigh from Indus to the Pole."

To preserve such an instrument from destruction is well worthy of the noblest aims of science; and such is the object

^a See Bell's "Essay on the Human Hand," Bridgewater Treatise.

proposed by the excision of the elbow-joint. It is an operation recommended to the practical surgeon by the fact that it has been attended with a large proportion of success, not alone in being rarely followed by fatal consequences, but in preserving a useful, efficient, and irreparable member from inevitable sacrifice. The excision of the elbow-joint may then fairly rank among the noblest and most successful of the innovations of modern conservative surgery.

The subject of this notice was a pale emaciated girl, about twelve or thirteen years of age. She presented all the characteristics of the scrofulous diathesis in a marked degree. Chains of enlarged scrofulous glands were prominent in her neck, which was disfigured with the cicatrices of former sores. The blue veins were conspicuous through her pale transparent skin, while her long dark eyelashes, and still darker eyes, with their expanded pupils, and her clubbed finger tops, all unmistakably bespoke a scrofulous constitution of decided type. She was much worn out by suffering. The left elbow was greatly swollen for some distance above and below the joint, was exquisitely tender and sensitive, and studded with several apertures, the mouths of sinuses, most of which led down to the carious bones which composed the joint. She could not sleep by night or eat by day, and the very passage of a foot across the floor increased her sufferings very much. Such was her condition on her admission into hospital.

The first object was to mitigate her suffering. She was given opiates freely, especially at night, but they were insufficient to procure her rest. Wine and nutritive aliments were supplied *ad lib.*, but she could not use food. Diarrhea threatened, night sweats were frequent, her sufferings were unremitting, and her remaining strength was rapidly ebbing. Amputation or excision of the joint appeared to be the only alternatives, and the latter was gladly acceded to by herself, the moment it was proposed.

The operation did not present any serious difficulty, and was effected, with the beneficent assistance of chloroform. A transverse incision was made by connecting two of the sinuses, which opened on opposite sides of the elbow. Two lateral incisions, parallel to each other were made, one on either side, so as to form with the first one a letter H. The intervening flaps were dissected upwards and downwards respectively, the ulnar nerve carefully drawn out of harm's way, and after isolating the diseased bones from the surrounding tissues, the carious portions of the humerus and ulna were divided with a fine narrow-bladed saw. The radius was

found to be free from disease. There was little blood lost. After adjusting the flaps, water dressings were applied, an opiate administered, and the patient replaced in bed.

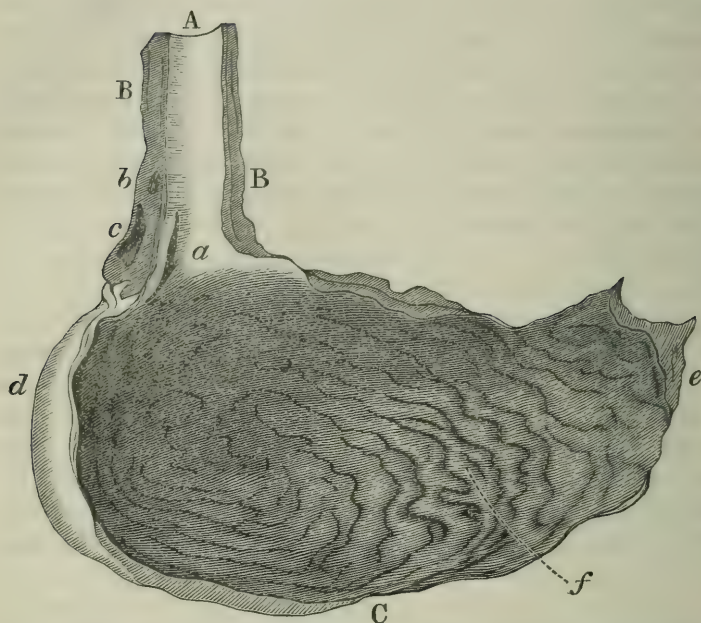
The immediate relief which ensued was very remarkable. She slept that night soundly, without her accustomed anodyne. All symptoms of hectic vanished. Her appetite returned at once, and corresponding improvement in her aspect and condition was rapid. The parts healed favourably, she was very soon able to sit up in bed and move the limb herself, and it was not long until she could leave her bed and walk about the ward. She ultimately enjoyed excellent use of the hand; could knit and do other work, and was able to raise and command the arm so as to comb her back hair with ease.

It is beyond all controversy that no mechanical contrivance could have formed an adequate substitute for the hand which was thus preserved; but what appeared to be the most remarkable circumstance was the prompt and immediate relief from suffering which followed the operation. Immediately before it the slightest touch or motion, even the vibration of the floor, caused intolerable agony; soon after it she could bear the limb to be moved by an assistant, and that night was able to enjoy refreshing sleep, without any artificial hypnotic. There can be no question that in this case the operation proved a present boon and an ultimate benefit.

ART. XVI.—*A Case of Rupture of the Œsophagus; with Remarks thereon.* By J. J. CHARLES, M.A., M.D.; Demonstrator of Anatomy, Queen's College, Belfast.

I LATELY made a *post mortem* examination of a man who had died rather suddenly, under circumstances which occasioned differences of opinion as to the cause of death. The rarity of the case and its importance to the pathologist and medical jurist led me to examine it with great care, and to refer to all the recorded cases of a similar nature which I could find. For the greater part of the history of the case, I am indebted to the courtesy of the medical gentleman who attended the deceased during his last illness.

History.—A. B., aged thirty-five, married, of a well developed muscular frame, but of intemperate habits. For a few days previous to his death he had eaten very little, but had been



A Drawing of the Interior of the Stomach and Lower Third of the Œsophagus.

A—Œsophagus laid open along the middle line behind.

B—Areolar sac around the œsophagus, containing black material.

C—Stomach.

a—Rupture of the œsophagus, and cardiac orifice of the stomach.

b—Small circular aperture in the left pleura.

c—Large irregular aperture, probably artificial.

d—Fundus of the stomach, where the mucous membrane is very soft and dark.

e—Pylorus of stomach, near which the mucous membrane is reddened.

f—Very prominent rugæ.

drinking spirits in excess. It was suspected that some of the whiskey he drank had been medicated.

At 8 o'clock p.m., feeling unwell after a light dinner, he went out to the garden, and began to exercise a horse. Here, whilst attempting to vomit, he felt something giving way in his inside. Having reached the house with difficulty, he fell on the floor in great agony. He was then assisted to his room and put into a warm bath. This seemed to relieve him, and he lay down in bed. A draught containing half a drachm each of aromatic spirits of ammonia, spirit of chloroform, and tincture of cardamoms was then administered, and a similar one given after an hour had elapsed.

He was greatly troubled with retching; and the pain which had been located in the neighbourhood of the left kidney, now ascended towards the stomach and back of the chest. Difficulty of breathing and intense thirst gradually ensued. The pain becoming more severe, turpentine fomentations were applied over its seat, and the patient was put into another warm bath, in which he remained twenty minutes. Half a drachm of liquor morphiæ with an equal quantity of tincture of hyoscyamus was then given, and repeated in two hours.

After the first dose he slept about half an hour, when the pain in the front and back of the chest, and the dyspnea increased to such a degree that turpentine stupes were again applied, but without relief. He then drank a cup of coffee. But he soon became delirious; and his pulse, which up to this time had kept pretty strong, suddenly failed. He died at 3.40 o'clock a.m.—about seven hours and a half after the fit of vomiting in the garden.

I may add that, in conversation with some of the friends of the deceased, after the *post mortem* examination, I was informed that from infancy he had been occasionally distressed with difficulty of swallowing.

Autopsy made nearly twenty-nine hours after death.

External appearances.—Body rigid and well nourished; features composed. An abrasion of the cuticle, seven inches by six, and of a brown colour, exists over the left scapula—probably the result of the application of a blister. On turning the body on its side, a quantity of dark fluid issues from the mouth, a portion of which is preserved.

Head.—Contents of cranial cavity apparently healthy, with the exception of slight congestion of the superficial veins and sinuses.

Chest.—The pleural cavities are partially filled with fluid, that

in the left being of a dark colour and having an offensive odour, and that in the right of the appearance of bloody serum.

Left pleural cavity.—Pressure on the stomach causes gas to bubble up through the fluid in this sac. The quantity of liquid, when removed from the body, is estimated at about two quarts. Moderately firm adhesions exist on the outer surface of the lung near the apex, almost corresponding in position to the external abrasion. Along the lower two-thirds of the posterior mediastinum, the pleura is blackened and so soft that it cannot be separated as a distinct layer. About two inches above the diaphragm a circular aperture, almost of the size of a fourpenny or sixpenny piece, appears in the pleura covering the œsophagus, through which, on applying pressure to the stomach, a dark grumous liquid flows into the pleural cavity. The rest of the pleura is healthy.

Right pleural cavity.—This sac contains about a quart of fluid, and the membrane appears healthy, save over the posterior mediastinum where it is slightly blackened. No aperture can be detected in it.

Lungs collapsed and somewhat congested.

Pericardium healthy. Walls of right side of heart congested, and those of the left anemic; otherwise the organ is healthy. Aorta natural.

Abdomen.—The peritoneum retains its usual lustre, and its serous secretion is of average amount. The stomach and intestines are filled with gas and contain little solid matter. The ileum is of a dark colour; but the rest of the intestines is nearly normal. Liver pale and slightly myristicate; gall-bladder distended with bile; kidneys very much congested; supra-renal capsules healthy. Urinary bladder almost empty.

The œsophagus and stomach, with a piece of the diaphragm, as well as portions of the intestines with their contents, and specimens of the fluids found in the pleural cavities, are removed for more detailed examination; but none of the vomited matter could be obtained.

Stomach and œsophagus.—An incision carried along the posterior surface of the stomach near the small curvature, and towards the cardiac orifice, and carefully prolonged upwards in the œsophagus almost in the mesial line posteriorly, exposes the interior of the stomach and œsophagus.

Stomach.—The contents consist of a black grumous liquid, of the colour and consistence of fine coffee grounds. The mucous

membrane of the cardiac end and body is very soft, and of a dark hue, especially in the former, where the membrane is so altered that the weight of the viscus is almost sufficient to tear it. Near the pylorus the mucous membrane is of a bright red tint; and though there are no obvious signs of congestion elsewhere, the substance of the organ, when held up to the light, is of an extremely red colour. In the body of the viscus the transverse and longitudinal rugæ are very prominent.

Œsophagus.—The mucous membrane of the lower third is slightly softened, and of a reddish colour close to the cardiac orifice. On the left side, near the posterior wall, there is a longitudinal fissure through all the coats. Superiorly, its edges are firm and well defined; inferiorly, for about the third of an inch, they are irregular and somewhat softened. This fissure reaches from immediately below the cardiac orifice of the stomach upwards for an inch and a half, but further in the mucous membrane than in the muscular and fibrous coats. No doubt it has been somewhat enlarged by manipulation. It leads into a space or kind of sac in the posterior mediastinum, which almost surrounds the lower third of the œsophagus, and extends from the cardiac orifice of the stomach to the roots of the lungs, the left one especially. This space contains grumous matter like that in the stomach, and its boundary on the inside is black, soft, and ragged. On careful examination it proves to be the ordinary areolar sheath of the œsophagus, distended and altered in character by the fluid lying in it. On the same side as the perforation in the œsophagus, but about two inches obliquely above it, the membrane (pleura) bounding the space is very thin, and presents the small aperture which has been already described as appearing in the left pleura. Below this perforation another, about an inch in diameter, is observed, which had not been previously detected. But this opening may have been artificially produced, at least in part, through the great softness of the tissues. The mucous membrane of the upper two-thirds of the œsophagus is of an opaque whiteness, and presents no abrasion of any kind. The mucous membrane of the pharynx and back of the mouth is feebly reddened.

Portions of intestines.—*Piece No. 1* (duodenum).—The mucous membrane is coated with a soft, yellow, pasty material. Close to the pylorus there is some grumous matter like that in the stomach.

Piece No. 2 (jejunum).—Eighteen inches long. Contains a fine

paste-like substance. Mucous membrane pale, with a few of its vessels injected.

Piece No. 3 (jejunum).—Four feet long. Contents of the upper half of the same nature as in Piece No. 2; but scattered through them are small black masses, some of which are as large as split peas. Near the middle there is a dark lump about the size of a small orange. In the lower half the black matter becomes more abundant.

Piece No. 4 (ileum).—Two and a half feet long. The mucous membrane is of a dark hue throughout. Contents sparing in quantity, of a mucous consistence, and dark in colour.

Piece No. 5 (large intestine).—One foot and a half long. Mucous membrane of the upper half of this portion reddened at intervals, and coated over with a grumous layer. In the lower half the mucous membrane is pale; and the contents whitish, small in amount, and with scarcely any black material.

MICROSCOPICAL AND CHEMICAL EXAMINATION.

Fluid in left pleura.—Acid, with a sour odour. Starch and oil globules occur in it.

Fluid in right pleura.—Neutral and very feebly acid. No starch granules are observed.

Grumous matter of stomach.—Acid, and consisting of blackened blood and starch, &c.

Black matter of intestines.—Of nearly the same structure and composition as the contents of the stomach, but much less acid in reaction—some of it, indeed, being almost neutral.

White substance found in intestines.—Neutral.

Mr. J. Hunter, F.C.S., late Chemical Assistant, Queen's College, Belfast, kindly examined the contents of the stomach and the fluids in the right and left pleuræ, but failed to detect any oxalic or arsenious acid, and obtained only so much sulphuric and hydrochloric acids as might be expected to be present in the gastric contents. A portion of the stomach also was boiled previous to examination, but without any different result.

REMARKS.

There is much room for speculation as to the nature of the above case. It might be suggested that the phenomena, which have been recorded above, are capable of explanation in any one of the three following ways:—They may have been due either

1. To the action of an irritant or corrosive poison;

2. To idiopathic acute gastritis, followed by cadaveric digestion ;
or

3. To a rupture of the œsophagus, with more or less *post mortem* solution.

In discussing these hypotheses the four following points must be considered and settled:—

1. The cause of the sudden accession of dangerous symptoms, and chiefly of the exclamation of the deceased immediately after vomiting in the garden, that “something in his inside had given way.”

2. The precise time of the occurrence of the fissure in the œsophagus—whether before or after death?

3. The source of the fluid in the pleuræ, especially in the right one.

4. The source of the grumous material in the stomach and in the various parts of the intestines; and the cause of its carbonaceous appearance.

I. *Mineral Poisoning*.—According to this view the blackening of the contents of the stomach and intestines, and the perforation of the œsophagus would be accounted for on the supposition that an irritant or corrosive poison had been taken; and the circumstances in favour of it might be stated as follows:—

1. The sudden accession of many of those symptoms which usually attend the administration of an irritant poison in a person previously in average health.

2. Death after a short but painful illness of seven and a half hours.

3. The carbonaceous appearance and tarry consistence of the contents of the stomach and of portions of the intestines, the congestion of the stomach, the reddening of the mucous membrane of the stomach near the pylorus, the blackening of it in the fundus and body, and the prominence of its transverse and longitudinal rugæ.

In opposition to this theory it may be urged:—

1. That we have no account of poison having been taken or administered—though the history of the case is far from being so complete as one might desire.

2. That, according to this view, we cannot assign a satisfactory reason for the feeling of rupture experienced by the deceased.

3. That there was no diarrhea—a symptom which attends the action of most irritant poisons—sulphuric acid and a few others excepted.

4. That there was no inflammation, corrosion, or abrasion of the mucous membrane of the upper two-thirds or three-fourths of the œsophagus, though the mucous membrane of the throat and fauces was feebly reddened; and that the edges of the fissure were straight and well-defined, and not irregular and blackened as after the action of a corrosive.

5. That no appreciable quantity of sulphuric acid or other irritant poison could be obtained on analysis of the contents of the stomach, intestine, or left pleura.

6. That Dr. Christison states that "The throat and gullet are very rarely penetrated, since the greater part of the poison must pass into the stomach, or be rejected by vomiting. Perforation from simple corrosion never occurs."^a

For these reasons, then, it is not likely that this is the correct hypothesis.

II. *Idiopathic Acute Gastritis, followed by Cadaveric Digestion.*—This rare disease may have supervened on a mild or sub-acute variety which had probably existed previously as a consequence of a persistent course of intemperance. The exciting cause may have been the last debauch; and it is very possible that the effect of the spirits imbibed was greatly intensified by such pernicious adulterations as oil of vitriol, &c., which are but too commonly present. The horse exercise taken while he laboured under this diseased condition of stomach may have been a still further exciting cause, and may have tended to shorten the premonitory stage of the disease. The symptoms from their first appearance increased in intensity, and were of the nature ascribed to acute gastritis, namely, acute pain in or near the epigastrium, persistent retching and vomiting (?), irrepressible thirst, and speedy collapse. Immediately before death effusion into both pleuræ occurred. After death the stomach was found to contain very little solid matter, but probably about a pint of fluid. Whether the gastric juice present in it possessed a more highly acid reaction than usual, as in the opinion of some,^b happens in cases of *post mortem* solution, must remain a subject of conjecture; but in the present inquiry this point is not very material. Besides the body of the deceased lay on a feather bed, and was well covered with a blanket and quilts, in tolerably warm weather (47° F. in the open air) for twenty-nine hours; so that it was favourably circumstanced for *post mortem* digestion.

^a On Poisons, p. 148. 1845.

^b Taylor on Perforation of the Stomach. Guy's Hospital Reports, Vol. iv. 1839.

The cardia of the stomach, especially near the orifice, seems to have been the most dependent part of the viscus, owing to its rotation upwards and forwards from the development of gases in its interior and in the intestines. There was, therefore, a preternatural tendency for the contents of the stomach to regurgitate into the œsophagus, and this tendency may have been increased by the contraction of the muscles during rigor mortis. Accordingly the lower third of the œsophagus and the fundus of the stomach were softened, and the latter blackened from the action of the gastric juice on the blood effused, and on that retained in the congested vessels of the mucous membrane. But the macerating influence of the gastric juice was mainly noticeable in the œsophagus, in which it produced the fissure already described. Having passed through this fissure, a portion of the gastric contents then made its way along the interior of the œsophageal sheath, and finally entered the left pleura, after dissolving a part of that membrane. Here it became mingled with the serous fluid present in that cavity, and on account of its diluted condition effected no further changes.

Some of the blood extravasated and blackened in the stomach was probably carried down the intestinal canal and lodged in the most dependent portions. But why the contents of the duodenum and of part of the jejunum should have been free from black matter, it is difficult to imagine, unless we consider the blood as effused principally from the intestines, and then darkened to some extent by sulphuretted hydrogen, &c.

To this theory, plausible as it may appear, there are three grave objections:—

1. It affords no explanation of the feeling of rupture complained of by the deceased.

2. The aperture or fissure in the œsophagus does not resemble one produced by gastric juice, for the perforation effected by this means has generally an irregular outline, ragged edges, and the mucous membrane around softened and digested; whilst in the present case the edges were, for the greater part, smooth, and the mucous membrane in the neighbourhood pale and moderately firm.

3. In acute gastritis, as in every other form of congestion of the stomach, according to Beaumont,^a the quantity of gastric juice secreted is smaller than normal. There is, therefore, less likelihood of the occurrence of *post mortem* solution of the œsophagus

^a Physiology of Digestion, pp. 99 and 100. 1847.

or stomach. Moreover, there was little solid food in the stomach immediately before death; so that, for this reason also, only a small amount of gastric juice would be thrown out.

4. Idiopathic acute gastritis is a very rare disease. Dr. Brinton coincides with Abercrombie in declaring that he never saw "acute general inflammation of the gastric mucous membrane save as a result of poison."^a And Dr. Copland affirms that acute gastritis is "rare as an idiopathic malady, unless when it is produced by poisons, or by substances which, from their quantity or condition, act as poisons, as the regurgitation of spirits, &c." "It occurs," he adds, "consecutively upon the milder forms of the disease, owing to persistence of the causes, or to improper treatment."^b

From the imperfect history I obtained of the case, I am unable to say whether the disease was "ushered in by chills or rigors, or was attended by febrile reaction." And, for the same reason, I am unaware whether the deceased complained of "excessive heat and burning in the throat, or of painful and difficult deglutition" before vomiting—symptoms which are sufficient, according to Dr. Copland,^c to diagnose acute gastritis caused by poison, from that arising idiopathically.

In connexion with this subject, I may here mention that Dr. Budd, in his admirable Croonian Lectures,^d gives numerous instances of cadaveric perforation of the Œsophagus opening into the left pleura; and he explains in a satisfactory manner why the left side of this canal should be digested in preference to the right.

Mr. T. Wilkinson King also records three cases of solution of the Œsophagus;^e and Dr. Barnes, a very interesting example of cadaveric perforation of the stomach.^f

We shall now examine the third theory, which, I believe, will be found more satisfactory than either of the preceding.

III. *Rupture of the Œsophagus, with more or less post mortem digestion.*—The rupture followed softening,^g which may have resulted from chronic inflammation of the stomach and lower third

^a Diseases of the Stomach, p. 86. 1864.

^b Medical Dictionary—Article, Stomach. Vol. iii., p. 914. 1858.

^c Opus Cit.—Article, Poisons. Vol. iii., pp. 336 and 337.

^d London Medical Gazette, Vol. xxxix. On this subject see also Wilk's Lectures on Pathological Anatomy, p. 279. 1859.

^e Guy's Hospital Reports, 1842.

^f London Medical Gazette, Vol. xli.

^g Simple softening described by Rokitsanski. Manual of Pathological Anatomy. Sydenham Society. Vol. ii., p. 11. 1849.

of the œsophagus. The continued excessive ingestion of alcohol probably induced in chronic gastritis or gastro-enteritis—the inflammation involving also the lower third of the œsophagus. Softening of the mucous membrane ensued as a consequence. During the fit of vomiting in the garden, rupture of the œsophagus, at its weakest part, occurred, thus causing the sensation of internal laceration which the deceased felt. Immediately preceding or accompanying the vomiting there may have been temporary spasm of this tube, resembling the “hour-glass contraction” of the uterus; and, under these circumstances, the contents would be impelled against its parietes close to the cardiac orifice. Dr. Copland speaks of spasm of the œsophagus as frequently attending inflammation of this canal.^a During the retching that followed, some of the gastric contents were forced into the sheath of the œsophagus, and thus occasioned considerable dyspnea. Subsequently the left pleura was perforated either before or, more likely, after death.

Whether gastric juice could digest living pleuræ is doubtful. Still it should be remembered that Dr. Pavy and others have shown that living tissues can be dissolved by this secretion, especially when they have a deficient circulation or are comparatively non-vascular.^b

In this as in the last hypothesis, we assume that effusion into the pleuræ occurred before death, and that the blackening of the mucous membrane of the stomach and intestines as well as of their contents, resulted from the action of gastric juice after death. The only essential difference between this theory and the last is, that in the present one we suppose rupture of the œsophagus to have occurred instead of idiopathic acute gastritis. In both we take into consideration the solvent properties of gastric juice after death.

It may be alleged in opposition to this hypothesis that rupture of the œsophagus is an exceedingly uncommon lesion. But laceration of the stomach in the vicinity of the cardiac orifice has been often observed as the consequence of apparently trivial causes. Dr. Christison refers to two cases in which rupture was the result of the accumulation of gases arising from depraved digestion. In one of them “a smooth laceration, *like an incised wound*, three inches in length, was found in the lesser arch of the stomach.”^c Here he

^a Op. Cit.—Article, *Œsophagus* (spasm of), p. 909.

^b Pavy on Digestion and its Disorders, p. 74. 1867.

^c Op. Cit., p. 117; and Copland's Medical Dictionary—Article, Poisons. Vol. iii., p. 335.

believes the rupture was due as much to weakened vital cohesion of the coats of the stomach as to the amount of the distending matters. He further states that "rupture of the stomach sometimes simulates in its symptoms the effects of irritant poisons. It is generally the consequence of over-distention combined with efforts to vomit. The cause of it seems to be that the abrupt turn which the gullet makes in entering an exceedingly distended stomach acts as a valve, so that the contents cannot be discharged by vomiting."^a Now if, with a relaxed cardiac orifice, we substitute temporary spasm of the œsophagus for the valve referred to by Dr. Christison, his explanation will apply to the present case. It should also be recollected that the fissure extended into the stomach for a short distance, so that the cases are more nearly parallel.

But we are not obliged to depend on analogy alone in support of the probable occurrence of rupture of the œsophagus. Several instances are on record. Mr. Dryden, Surgeon in Jamaica, relates the case of an officer who, after a fit of inebriation, on December 1st, 1787, was seized with nausea and inclination to vomit. To induce vomiting, he drank warm water. During the straining which followed, "he felt something give way internally which gave him the sensation as if he had received an injection of some liquid matter into the cavity of the thorax. He also brought up a little blood," and suffered acute pain "about the region of the stomach and abdomen." "The vomiting now ceased, and was succeeded by thirst, great heat in the stomach and throat, constipation, and restlessness." Emphysema of the neck then manifested itself. After passing small, lumpy stools, the pain moved to the left side, though slight pain was also felt on the right side. The breathing became very laborious, "accompanied with heavy sighs and groans." Death took place eight or ten hours after the first attempt at vomiting. [The exact length of time from the first seizure is not mentioned in the report.] At the *post mortem* examination a longitudinal laceration was discovered in the œsophagus, just before it passes through the diaphragm, large enough to admit the fore and middle fingers. About a gallon of a mixture of wine, water, and food was contained in the left pleura, and nearly two quarts of the same kind of fluid in the right pleura. [The period which elapsed between death and the *post mortem* examination is not stated.] Mr. Dryden concludes that the "action of vomiting was the sole cause of this rupture."^b

^a Op. Cit., p. 116.

^b Medical Commentaries Edinburgh. Decade 2. Vol. iii. 1788.

The resemblance between this case and the one forming the subject of the present inquiry is so obvious as to need no comment.

Böerhaave^a details the case of a robust, gouty baron who, after vomiting, complained of a feeling of internal laceration. He died after twelve hours of intense agony. The œsophagus was found ruptured transversely, a little above the cardiac orifice of the stomach, and the left pleura contained fluids from the stomach. Dr. Monro quotes this case as an example of rupture occurring during life;^b but Mr. Wilkinson King thinks it was one of cadaveric solution.^c Dr. Monro further says that an instance like that of Mr. Dryden's was communicated to him by Dr. Carmichael Smyth.^d

Mr. Wilkinson King describes the case of a Mr. Curtis, who had been a "very hard drinker for several years." Whilst at a public supper, he became sick and vomited slightly. He then felt a severe pain in the epigastrium. His face, throat, and chest shortly became emphysematous;^e dyspnea set in, and gradually increased. He sank after fourteen hours of great distress. A large rent was detected in the œsophagus where it passes through the diaphragm, and there were found "portions of food at the posterior part of the chest." The fundus of the stomach was softened by digestion; and Mr. King states that it is not possible for him to say "if there be a rupture, where the *post mortem* solution ends and where the laceration begins." He believes that "the probabilities considered there was no rupture;" but that death resulted from "sudden inflammatory tumefaction of the larynx."^f Dr. Habershon, however, details the history and *post mortem* appearances of Curtis as an instance

^a Van Swieten's Commentaries upon Böerhave's Aphorisms. Vol. ii., p. 102. Edinburgh, 1786.

^b Morbid Anatomy, p. 311. 1811.

^c Guy's Hospital Reports, 1843.

^d Op. Cit., p. 311.

^e The superficial emphysema in these cases probably originated in the rupture of an air cell or bronchus, from the straining efforts made in vomiting. The air having thus reached the posterior mediastinum travelled upwards along the œsophagus to the neck, whence it diffused itself in the subcutaneous areolar tissue.¹ Mr. Hilton has described traumatic emphysema as occasionally arising in this manner.² Dr. Lyons gives instances of emphysema occurring in the course of measles, chronic bronchitis, &c.,³ which well deserve perusal. The cause of the extravasation of air in his cases is, in all likelihood, the same as that I have just mentioned.

^f Guy's Hospital Reports, 1843.

¹ Holmes' System of Surgery. Vol. II., p. 361. 1861.

² Erichsen's Surgery, p. 392. 1864.

³ Clinique, Dublin Medical Press and Circular, September 5, 1866.

which "warrants the belief that sometimes rupture of the coats of the œsophagus takes place during life."^a Dr. Copland coincides with Dr. Habershon's view of the case.^b

Other instances of rupture of the œsophagus have been published by Zeisner,^c Guersent,^d Boulilaud,^e Vigla,^f Reil,^g Kade,^h Thilow,ⁱ Meyer,^k Oppolzer,^l and Sédillot.^m

The views of Professor Bamberger on rupture of the œsophagus may be here appropriately recorded. "Rupture of the œsophagus is in general a very rare phenomenon. In a few cases laceration has been observed in a perfectly healthy condition of the œsophagus, during bodily exertion or violent retching. . . . Perforations and ruptures may appear in either the lower or upper part of the œsophagus, but in the large majority of cases, in the former. The size and form of the opening are various, and depend upon the immediate cause. . . . The phenomena of rupture are, according to Oppolzer, undefined. Suddenly, during vomiting, there occur severe pain, vomiting of blood, agony, and oppression; whilst the food and drink rush into the mediastinum. We can, therefore, diagnose this disease only when, during a fit of vomiting, violent pain in the course of the œsophagus sets in, when vomiting can no longer take place, and when, at an earlier period, symptoms have been present which point to an affection of the œsophagus."ⁿ

The opinion of Dr. Gross on this subject is almost similar. He states that "laceration of the œsophagus generally takes place during the act of vomiting, without any antecedent structural lesion. The rent is commonly transverse, or somewhat oblique, and varies in extent from a few lines to half an inch or more. The inferior portion of the tube gives way more frequently than the upper or

^a Observations on Diseases of the Alimentary Canal, p. 50. 1857.

^b Op. Cit. Article : Œsophagus (Rupture of), p. 908.

^c Haller's Disput. Med. 7 Band. Seite 629.

^d Bulletins de la Faculté de Méd. Tome 1.

^e Archiv. Génér. de Méd. Tome 1, p. 531.

^f Archiv. Génér., 12 Band, s. 129 und 314.

^g Memor. Clinie fasc. 1.

^h De Morb. Ventric. Halæ, 1798.

ⁱ Baldinger Neues Magaz. f. Aerzte. 12 Band. Seite 2.

^k Med. Kereinsz. 1858. Nos. 39-41.

^l Wein. Med. Wochschr. 1851.

^m Recueil Periodique, tome 7, p. 194, quoted by Dr. Copland in his Medical Dictionary, Vol. ii., p. 908. 1844.

ⁿ Handbuch der Speciellen Pathologie und Therapie. 6 Band. Erst. Abtheilung, s. 97. 1864.

Quoted by Professor Bamberger, in Virchow's Handbuch der Speciellen Pathologie und Therapie. Abtheilung, s. 97.

Quoted by Dr. Foerster, in Handbuch der Speciellen Pathologischen Anatomie. 2 Band., s. 69.

middle; sometimes the rupture is situated immediately above the cardiac extremity of the stomach. The accident is characterized by violent pain, with symptoms of sinking, and usually proves fatal in from 36 to 48 hours."^a

In addition the reader may refer to the works of Lebert,^b Foerster,^c and Niemeyer.^d

Before concluding this paper I wish to express my sincere gratitude to Professor Redfern for his kindness in bringing the above case under my notice, and for his valuable assistance, at all times most willingly accorded, during the progress of my investigations. I beg also to tender my thanks to Professors Cuming and Reid for useful suggestions.

^a Elements of Pathological Anatomy, p. 499. 1857.

^b Handbuch der Practischen Medicin, 3 Auf., 2 Band., s. 295. 1862.

^c Handbuch der Speciellen Pathologischen Anatomie, 2 Band, s. 69.

^d A Text Book of Practical Medicine. Translated by Humphreys and Hackley, Vol. i., p. 469. 1869.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Lectures on Obstetric Operations, including the Treatment of Hemorrhage, and forming a Guide to the Management of Difficult Labour.
By ROBERT BARNES, M.D., Lond., F.R.C.P. London: John Churchill and Sons. 1870. 8vo, pp. 526.

THESE lectures have been so frequently noticed in this Journal that it is almost unnecessary to enter on a formal review of them. We believe them to give the most explicit, definite, clear, and complete, and withal simple description of obstetric operations ever published in the English, or indeed in any other language; descriptions not projected from the inner consciousness, but the result of practical experience, guided by the closest study of principles, and the fullest apprehension of the obstacles to be met with, and the mechanism by which these are to be overcome. In their present form these lectures form an octavo volume of more than 500 pages. They treat of obstetric operations, of uterine hemorrhages, and of placenta previa.

The first lecture is introductory, and on the selection of instruments; then we have four on the use of the forceps, giving a very full account of the method of using this instrument. It is to be regretted that Dr. Barnes has not defined more accurately what he means by the terms "long" and "short" applied to forceps, for though there is an improvement since his attention was called to the subject by Dr. Beatty in the correspondence that took place when the lectures were first published, in the *Medical Times and Gazette*, there is still much uncertainty and obscurity in the use of these terms. Dr. Barnes adheres to his love for forceps with a second or pelvic curve, but in the excess of his enthusiasm refutes his own decision by the reasons he adduces in its support. The "objections to the single-curved forceps, short or long," stated at pp. 47-8-9, are so utterly without foundation, and have been so fully answered in this Journal and elsewhere, that it is not necessary to go into them again; and as Dr. Beatty has already remarked

the diagram given by Dr. Barnes (p. 48), to prove the superiority of the curved forceps, affords in itself the most conclusive reason why the straight forceps should always be preferred. This controversy, like many others, will probably never end. The fact is that either instrument will, in skilful hands, do all that ought to be done with a forceps, and the question as to which is the most easy of use and the safer, involves so much of the skill of the operator that it must always remain open, but few will, we opine, adopt the practice of an eminent teacher in Edinburgh who stated at the late meeting of the British Medical Association, at Newcastle, that he used the forceps with the curve directed backwards.

The division of forceps operations into "high," "middle," and "low," according as the head lies at the brim, in the cavity of the pelvis, or on the perineum, simplifies very much the consideration of the subject, and we regret that Dr. Barnes had not adopted it more decidedly, but he has described all the steps of the operation, and the various modifications necessary to meet particular circumstances so clearly that we are little inclined to find fault. We are glad to find the stereotyped rule that the ear must always be felt before the forceps is applied, boldly declared unnecessary. The other standing order that the head must be a certain number of hours without advancing before we dare to give relief, is not even deemed worthy of mention—a course in which we cannot fully concur, for it is so generally laid down by routine writers and in text-books, and is productive of so much mischief, that we would lose no opportunity of showing its danger. There is no statement in the book more true or more important than that with which the lecture on craniotomy terminates—"Properly speaking the mortality from the forceps is *nil*. Women die because the instrument is used too late." The use of ergot is by some looked on as safer than the forceps, and it is certainly more easy; but let us hear Dr. Barnes as to the advisability of using it, and let us in leaving this section of the book advise all who, from timidity or want of confidence in themselves, give ergot instead of delivering with the forceps, to study these lectures and make themselves familiar with this instrument.

"But when you have given ergot you are likely to be in the position of Frankenstein. You have evoked a power which you cannot control. Ergotism, like strychnism, will run its course. If it acts too long or too intensely, you cannot help it. The ergotic contraction of the uterus, when characteristically developed, resembles tetanus. Then, woe to the

mother if the cervix does not yield, if the pelvis is narrowed, if, in short, any obstacle should delay the passage of the child. And woe to the child itself if it be not quickly born. I very much prefer to use weapons that obey me, that will do as much, or even less, than I wish. I fear to use weapons that will do more."

Ten lectures are devoted to this operation of turning, embracing a very full account of the "spontaneous evolution" of Denman, and the spontaneous expulsion of Douglas; both terms are, however, taken exception to by Dr. Barnes. The process described by Denman is, he says, a true *version* or *turning*. All German, French, Italian, and Dutch authors apply to this process the term spontaneous "version," "*versio spontanea*." It might, Dr. Barnes says, be called natural version to distinguish it from artificial version effected by the hand of the obstetrician. All continental authors, he says, likewise call Douglas's process by the name "spontaneous evolution" The process being one of unfolding, as it were, of the doubled up fœtus, Dr. Barnes thinks it of great importance to bring one phraseology into harmony with that of our Continental brethren, and of still more importance to bring it into harmony with nature; and as he says it is clear the change should be made by us, he uses the term "version" for the process described by Denman, and "evolution" for that described by Douglas. Both processes are very fully described, and their mechanism explained by diagrams drawn by the author himself; and indeed the book is most copiously illustrated in this way, the drawings being most expressive, clear, and faithful. Having shown the accuracy of Denman's description of the facts, and described the mode in which version and evolution take place, Dr. Barnes proceeds to apply the knowledge of the mechanism of these processes to the practice of artificial version and evolution. After describing the various methods of turning, bi-polar turning of the head, by the breech and by the feet, an account is given of the management of certain difficult breech presentations, locked twins, dorsal displacements of the arm, double monsters, and of delivery in imitation of spontaneous evolution, by evisceration and decapitation. Turning in contracted pelvis is also considered and forcibly advocated, and in the following passage an attempt is made to define the extreme limit of pelvic contraction in which the operation would be justifiable:—

"Can we define with any precision the conditions as to degree of pelvic contraction that are compatible with the birth of a living child?

The question is not easy to answer; nor is it important to be able to answer it very precisely. The great fact upon which the justification of the operation rests is this: many children have been delivered by it alive, with safety to the mother. We know accurately only one element of the problem—namely, the degree of contraction of the pelvis. The other element, the relative size and hardness of the foetal skull, we can but estimate. *We must assume, in many cases, a standard head.* With this assumption the practical question is reduced to this: *What is the extreme limit of pelvic contraction justifying the attempt to deliver by turning?* In other words, this means: What is the narrowest pelvis that admits of the passage of a normal head? This is answered chiefly by experience. It is not to be answered by *à priori* reasoning like that urged by Dr. Fleetwood Churchill, who says, even in his last edition: ^a—“The bi-mastoid diameter in the six cases measured (by Dr. Simpson) varied from $2\frac{6}{8}$ to $3\frac{2}{8}$ inches, and a living child can pass through a pelvis of $3\frac{2}{8}$ inches antero-posterior diameter, with or without the forceps. With a pelvis of this size, then, the operation is unnecessary; and if the antero-posterior diameter be less than $2\frac{6}{8}$ inches, the operation would be impracticable. These, then, are the limits of the operation; for us to attempt to drag a child through a smaller space would be unjustifiable.”

“To this statement of the case serious objections may be taken. The proposition that a living child can pass through a pelvis with an antero-posterior diameter measuring $3\cdot25''$, with or without the forceps, can only be accepted with considerable qualifications. I claim to speak with the confidence drawn from large experience, when I say that a head of standard proportions and firmness will hardly ever pass a conjugate reduced to $3\cdot25''$ without the forceps, and very rarely indeed with the forceps—that is, alive. I might even extend the conjugate to $3\cdot50''$, and affirm the same thing. The compressive power of the forceps, unless very long sustained, is not great, rarely great enough to reduce a bi-parietal diameter of $4\cdot00''$ to $3\cdot50''$ without killing the child. My opinion, then, is, that a standard head, especially if it happen to be a female head, which is more compressible than a male one, *may be drawn* through a conjugate of $3''$, but not with much prospect of life; and that the proper range of the operation of turning is from $3\cdot25''$ to $3\cdot75''$, at the latter point coming into competition with the forceps. I believe no one advocates resort to turning when the conjugate measures less than $3''$.”

Craniotomy is the subject of the next lecture. Under this heading six methods of delivery are considered. 1. By the crotchet. 2. By turning of the perforation. 3. By craniotomy-forceps, by which, according to Dr. Barnes, a full-sized head may be delivered with

^a “The Theory and Practice of Midwifery,” 1866.

safety to the mother, through a pelvis measuring even less than 2'00'' in the conjugate, provided that be 3'00'' in the transverse diameter, four by the cephalotribe. Of this instrument Dr. Barnes seems to have not yet attained a full knowledge; and though he shows in several places through the book a very high appreciation of its powers as lessening the number of cases where the Cæsarian section may be necessary, he proves that he does not yet know the full value of the instrument when he says that he doubts much whether cephalotripsy can carry the possibility of safe delivery at all beyond the point attained by the craniotomy-forceps. The assertion that the all-essential point is that the cephalotribe shall be able to compress and even break down the base of skull is further evidence of the same fact. The casts exhibited by Dr. Kidd at the Leeds meeting of the British Medical Association prove that the action of the instrument is to flatten and compress the vault of the cranium, and to turn the base of the skull edgeways, and not to break down the base. At page 309, Dr. Barnes gives a drawing of one of these casts which illustrates this very point, but, notwithstanding that one of the most interesting features in connexion with the casts is that they were taken from the heads exactly as they were delivered by the cephalotribe, and before the instrument was removed. Dr. Barnes has taken the rather strange liberty of substituting Dr. Braxton Hicks' cephalotribe for that of Dr. Kidd, and has thus made rather a fancy sketch of the drawing, but not, we are bound to say, without making full acknowledgement. We are very glad indeed to see Dr. Barnes enlisted among the advocates of cephalotripsy, and hope that what he says of it will give an impulse to the adoption of the operation. We believe that this will, before long, supersede all other plans of delivering the head after perforation, and that, in doing so, it will greatly diminish the mortality of labour when the pelvis is deformed. Three times Dr. Kidd has delivered with perfect safety one woman, the conjugate diameter of whose pelvis was little more than $1\frac{3}{4}$ inches. Dr. Braxton Hicks has applied it where there was not more space than $1\frac{3}{4}$, and in Dr. Barnes' own case, the conjugate diameter was only 1'50''. It will be seen from the following extract that, notwithstanding his love for curved instruments, Dr. Barnes' experience in this case has convinced him that his curve of the cephalotribe "should be very slight":—

"The Powers of the Cephalotribe.—The all-essential point is that it shall

be able to compress and even crush down the base of the skull. A secondary property which it is desirable to possess is that of holding during extraction. The crushing power can be attained in sufficient perfection, and with a gain in the facility of handling, if the instrument be made much less formidable in bulk than are most of the Continental cephalotribes. Three good modifications have been constructed here. Sir James Simpson's is the best known. He insists upon a pelvic curve in the blades as being less likely to slip than straight blades. Dr. Kidd's, of Dublin,^a is the best type of a straight-bladed cephalotribe. Dr. Kidd insists strongly upon the advantages of long straight blades on the following three grounds:—First, straight blades admit better of the head being rotated whilst in the grasp; secondly, they are easier to introduce; and lastly, they hold more securely. Dr. Braxton Hicks has modified Sir James Simpson's cephalotribe, producing a very handy and efficient instrument. He preserves a moderate pelvic curve, and adapts a very convenient screw to the handles as a crushing power. I believe that to seize a head above the brim, as is necessarily the case where crushing is required, the blades should be curved; but this curve should be very slight, otherwise the inconvenience in rotating or shifting the relation of the instrument to the pelvis referred to by Dr. Kidd will be felt."

4. Delivery by the forceps-saw of Van Heuvel is next described, and then Dr. Barnes passes to the 5th method—that is, the use of the *écraseur*, as described by himself, but which he has not yet had an opportunity of putting in actual practice. The head having been perforated and fixed against the brim by an assistant, the steps of the operation are as follows:—

"The crotchet is next passed into the hole made by the perforator, and held by an assistant so as to steady the head. A loop of strong steel wire is then formed large enough to encircle the head. The elasticity of the wire permits of the loop being compressed by the fingers so as to make it narrow enough to slip through the cervix uteri and the chink of the pelvic brim. The loop is thus guided over the crotchet to the left side of the uterus, where the occiput lies. The compression being removed, the loop springs open to form its original ring, which is guided over the occiput, embracing all the posterior segment of the head, as in Fig. 90. The screw is then tightened. Instantly, the wire is buried in the scalp; and here is manifested a singular advantage of this operation. The whole force of the necessary manœuvres is expended on the fœtus. . . .

"When the posterior segment of the head is seized in the wire-loop, a steady working of the screw cuts through the head in a few minutes. The loose segment is then removed by the craniotomy-forceps.

^a See "British Medical Journal," October, 1867.

"In minor degrees of contraction, the removal of the occipital segment is enough to enable the rest of the head to be extracted by the craniotomy-forceps. But in the class of extreme cases in which this operation is especially useful, it is desirable still further to reduce the head, by taking off another section. This is best done by re-applying the loop over the anterior side of the head as seen in B, Fig. 90. The wire seizes under the lower jaw beyond the ear. When the screw is worked, the wire has to cut through the base of the skull, dividing the sphenoid bone. The segment thus made is removed by the craniotomy-forceps.

"The small part of the head still remaining attached to the trunk offers no obstacle. It is useful as a hold for traction. The craniotomy-forceps now seizes this firmly, and you proceed to deliver the trunk."

Cæsarian section is the next subject considered, and is justly looked upon as "the last refuge of stern necessity."

"I repeat," says Dr. Barnes, "with all the emphasis that conviction based upon experience dictates, that delivery by the natural passages, either by cephalotripsy, by the craniotomy-forceps, or by my new method of embryotomy, if the conjugate diameter measures 1.50'', is perfectly practicable, and with a presumption of safety to the mother much greater than that attending the Cæsarian section."

We next come to a lecture on the induction of premature labour, in which the use of the elastic bags for dilating the os is fully described, and then we have lectures on hemorrhage and on placenta previa, which conclude the volume.

To attempt a full analysis of such a work as this, in the space at our command, would be useless. It embraces the whole range of operative midwifery; not only are the operations described, but also the principles on which it is founded. We regard the work as one of the most valuable contributions that has ever been given to obstetric medicine.

WORKS ON MATERIA MEDICA AND THERAPEUTICS.

1. *On the Present State of Therapeutics: with some Suggestions for placing it upon a more Scientific Basis.* By JAMES ROGERS, M.D. London: J. Churchill and Sons. 1870. 8vo. Pp. 232.
2. *Observations on Therapeutics and Disease.* By D. CAMPBELL BLACK, M.D. London: J. Churchill and Sons. 1870. Pp. 48.

3. *A Handbook of Therapeutics.* By SYDNEY RINGER, M.D. Small 8vo, p.p. 485. London: H. K. Lewis. 1869.
4. *The "Modified Examination" of the Pharmaceutical Society.* By F. HARWOOD LESCHER. Second Edition. Pp. 71. 1869.
5. *An Introduction to the Elements of Pharmacy, or the "Major and Minor Examinations."* A Guide to the principal points in *Materia Medica*, Botany, Chemistry, Pharmacy, Prescriptions, and Practical Dispensing. By F. HARWOOD LESCHER. London: Churchill. 1869. Pp. 199.
6. *On the Administration of Chloroform and Nitrous Oxide.* By C. SQUAREY, M.B. London: J. Walton. 1869. Pp. 44.
7. *Diaphoresis.* By CHARLES CLARK, M.A., Cantab., M.R.C.S.E. London. 1869. Pp. 16.

THE relation of practice to theory, of experience to reason, constitutes now, as it ever has been, the main question in medical science, and the improvement of therapeutics is admittedly the great end and aim of practical medicine. From the natural course of events and from the increased spread of the spirit of questioning, it has come to pass that a widespread distrust of our powers of guiding or controlling disease by drugs has arisen. At such a period of hesitation and doubt an honest and unbiassed statement of matters as they stand is primarily of great importance, and the labours of thoughtful men, such as Dr. Rogers, can scarcely fail to have their fruit in defining more clearly the true principles on which therapeutics depends. It is not one of the least merits of Dr. Rogers' singularly able and dispassionate work, that while he continues a member of the "old school," he is fully conversant with the tenets of its rival homœopathy, and has enjoyed unusual opportunities of watching the practical working of that system. He approaches its consideration in a fair and courteous spirit, openly acknowledges the faults or deficiencies of our present methods, and impartially examines the grounds, perhaps we should call them quicksands, on which our faith has hitherto rested. The origin of Dr. Rogers' essay dates from the commencement of his professional career, when he was much struck with the reported success of homœopathic treatment. Since that period some trustworthy reports have been furnished by physicians attached to homœopathic hospitals, which leave

no reasonable doubt about the large proportion of recoveries that occur in their practice. It is evident that the recoveries referred to in these reports must be ascribed either to the curative powers of the organism itself, or to that aided by the action of the drugs. A complete knowledge of the action of one of the two agents engaged in the cure of any case of disease in which medicine has been employed, would render it not very difficult to assign to the other its share in the production of it. Dr. Rogers sets himself to prove that they cannot be accounted for on the latter supposition, both by *à priori* reasoning, and by a comparison between the results afforded by the homœopathic and expectant treatment. We thus have reason to conclude that the recoveries were effected by the natural resources of the organism, and that the histories of cases treated homœopathically may, therefore, be considered as so many illustrations of the natural course of disease. Hence we can understand that the large proportion of recoveries which take place under homœopathic treatment may be a fact, although the principles of the doctrine are unfounded, and, in so far as the natural history of disease has been elucidated, "orthodox" medicine is a debtor to homœopathy. To follow Dr. Rogers fairly through his arguments would demand more space than is at our disposal, and we must refer our readers to the work itself, which deserves attentive study, as one of the landmarks in the present phase of therapeutics, a suggestive book, full of wise teaching for those who will learn.

The volume is divided into three parts; the first, occupying more than half the entire text, is mainly taken up with a searching inquiry into the supposed law of "*similia similibus curantur*." The examination of this fundamental homœopathic principle is carried out with great ability and irresistible logic, and the conclusions arrived at, drawn from experimental and natural evidence are more than justified by the data on which they rest.

To use the language of another writer, the "homœopathic doctrine offers the strangest assemblage of assertions devoid of all truth, audacious paradoxes, and flagrant contradictions; in a word, is a challenge to the credulity of mankind." Since the publication of Sir J. Simpson's striking computations regarding the actual amount of the infinitesimal doses or homœopathic dilutions any further proof of their absurdity is superfluous, and the dynamization hypothesis is even now abandoned by some of the abler homœopaths. The second part is devoted to a detailed comparison of the results obtained in the treatment of disease by medical men of

the old school, and by homœopathic practitioners. The tests selected are acute rheumatism, intermittent fever, cholera, typhus fever, and pneumonia; and though it might be questioned whether these are the best examples for that purpose, they are the only ones on which reliable statistics could be obtained. From such limited and imperfect data it is not pretended that a decisive conclusion can be arrived at regarding the comparative value of homœopathic and non-homœopathic treatment; still Dr. Rogers thinks that we may safely draw these two important conclusions:—1st, That in the diseases examined, with the exception of intermittent fever, the results of homœopathic treatment in hospitals have been about equal to the most satisfactory non-homœopathic; 2nd, that the results of homœopathic and non-homœopathic treatment, in which little or no medicine was employed, have been nearly the same; or, in other words, that drugs in the doses usually administered by homœopathic practitioners have not appeared to exercise any decided influence on the progress of disease. No doubt this opinion is not novel, and when so distinctly laid down is humiliating to our art, yet it is better to recognize at once how far we are from the light than to continue to grope on in darkness. It is too true that “as yet we have no system of therapeutics based on rational or well-established principles,” and no conscientious or intelligent medical man can ponder over the history of *Materia Medica* “without resolving to abandon the chaotic polypharmacy of the old school, and trying to ascertain by proper investigations what drugs really do accomplish in the cure of disease.”

The third and shortest, but certainly not the least valuable, part of Dr. Rogers' essay embodies some suggestions for placing therapeutics on a more scientific basis than it is at present. Recognizing the aid that physiology, pathology, and chemistry, especially the latter, have given, and may be expected to furnish to rational therapeutics, particular stress is rightly laid upon the importance of acquiring a true knowledge of the natural course of disease. It is undoubtedly of the most vital importance to estimate aright the natural tendency of the diseased organism to return, under favourable circumstances, to a healthy state; and it has been well said that “the wise physician treats the healing power of Nature as the sunflower the sun, he follows it till it becomes invisible.” But we think that Dr. Rogers attaches too great weight to the influence exerted by homœopathy upon regular practice, and has

scarcely allowed sufficiently for the impetus which has been given to the more general acknowledgment of the sanative powers of nature in these countries within the last few years by the improved state of physiological and pathological science. Few educated physicians believe now that they can curtail by drugs the course of most acute diseases which run a definite course and tend to spontaneous recovery. Yet, though such an idea is happily fast receding from practice, much still remains for the interference of art in meeting special symptoms, and controlling intercurrent complication. Since our ignorance of the curative resources of the organism, and of the healing powers of drugs have been, and still are the chief sources of error in therapeutics, and the chief obstacles to its improvement, it follows that the foundation stone for positive knowledge must be laid in more accurate investigations into the properties of drugs. This will be best carried out by carefully conducted trials on healthy individuals, checked by collateral trials on the lower animals, and on patients suffering from diseases whose diagnosis, general course, and variations are tolerably well known. In this way we would become acquainted with the special "sphere of action" of the most important drugs, and learn to mark the elective affinities which particular drugs have for certain organs or tissues: It is in this direction that we are probably to look for a solid basis for our future therapeutics. Hitherto it has been almost exclusively the custom to endeavour to acquire a knowledge of medicines by instituting trials with them in disease, a method which has borne little fruit in return for the labour bestowed upon it. To the homœopaths, and to Hahnemann in particular, before he was carried away by the delusion of infinitesimal doses, belongs the credit of actively pushing forward the proving of medicines on healthy individuals, recommended by Störck, Alexander, and Haller; and it is strange that, with the exception of Professor Jörg, the Allopathic Proving Society of Vienna, and Rademacher's pupils, no provings of worth have been made by non-homœopathic physicians. Within the last two years, indeed, Dr. J. Harley has shown the value of this mode of inquiry in his admirable work on the action of opium, belladonna, and hyoseyamus, and it is greatly to be desired that others should undertake similar researches. *Sine experientia vana omnis theoria, bella sit utcunque.*

The candour and impartiality of the author throughout the whole work are so remarkable that it seems ungracious to take exception to any of his positions. The growth of the essay seems to have

been gradual, and the conclusions arrived at are evidently the result of mature consideration. Yet we cannot but think that Dr. Rogers fails to appreciate sufficiently the positive advances which have been made within the last twenty years, both in the doctrines and art of therapeutics. We do not say that there is a foreshadowing of any broad and universal principle comparable in simplicity to the basal homœopathic dogma, nor is it probable that such will ever be found. But we could point to many important gains, both positive and negative, if the expression may be allowed, recently made by therapeutics. Certain injudicious or noxious lines of treatment have been abandoned, *e.g.*, excessive mercurialization, and if fewer remedies are now used we have gained in precision what we have lost in quantity. The rational application of chemical investigation into the nature and mode of action of drugs is already yielding promise of golden fruit; and as striking examples we can refer to Crum Brown and Fraser's important papers on the connexion between chemical constitution and physiological action, to the speculations of Broadbent, to the labours of Bence Jones and Richardson, to the production of apomorphia, and, lastly, to Liebreich's pregnant discovery of the therapeutical value of chloral, and its congeners, bromal and iodal. We could also instance the improved treatment of skin diseases consequent on a more accurate acquaintance with their pathology, *e.g.*, the parasitic affections, and the modern views as to the etiology and pathology of pulmonary phthisis will lead to a more hopeful view of that hitherto hopeless malady. Hypodermic injection is an aid for which we cannot be too grateful, and a discrimination between the effects and uses of the direct and induced electric currents has led to most important results in the treatment of such formidable diseases as epileptiform neuralgia, infantile paralysis, and progressive muscular atrophy.

For the rest we have nothing but unqualified praise for Dr. Rogers' book, and we cannot doubt that the principles advanced in it, being founded on reason, will be those which will ultimately prevail to the lasting advantage of medical science.

Of Dr. Black's pamphlet it is difficult to speak with equal satisfaction. In some respects it is clever, and his ideas tend in the right direction, but it seems as if his thoughts outran his powers of developing them; the reasoning does not keep pace with the premises laid down. Many grave exceptions could be taken to the author's chemistry, to his physiology, and to his pathological

principles; and he fails, we think, to lay a sound foundation for "a more rational classification of diseases and remedies." The author holds that the great desideratum in therapeutics is to make our remedial agents subserve to those chemical and physiological conditions which constitute life, and the chief merit of Dr. Black's observations lies in the recognition of this important principle.

Treatises on therapeutics are of two kinds. They may be written either for the purpose of unfolding a general theory of the action of medicines, or they may aim only at detailing the special applications of the principal medicines which have been sanctioned by experience.

Dr. Ringer's work is of the latter class, and it is essentially an epitome of empirical observations, partly collected from others, and largely added to by his own experience. Regarded from that point of view the book contains many valuable hints and practical rules as to treatment, and we would especially mark, as deserving of commendation, the articles on arsenic, alcohol, chloroform, belladonna, &c.

As it is especially intended for students and the junior members of the profession, we do not look for any elaborate scheme of classification, but every writer must adopt some scheme, and we cannot heartily endorse Dr. Ringer's choice of following Buchheim as his model. His arrangement is based upon the idea of associating together those substances which have a common mode of action, or contain some principle common to them all. So far, this is quite right; but we think that some more definite basis of primary grouping should be adopted, and in particular that of general and local remedies, under which the special families of medicines would appropriately come in.

We give the headings of the first few sections, which will show the arbitrary sequence of articles which the author has preserved in this book:—Oxygen, On the internal use of Water, Cold, Ice, Warm and Hot Baths, Peroxide of Hydrogen, Carbon, &c.; and again, further on, we have consecutively Senega, Benzoin, Anthelmintics, Poultices, Enemata.

If the effects of general influences—such as heat and cold—are discussed, why omit those of mineral waters, or of an agent of such admitted powers as electricity in its various forms? An introduction on the general therapeutics of diet, exercise, cold, heat, electricity, depletion, &c., would most fitly precede the more complex

study of the action of particular drugs, and without committing himself to any strict system of classification, some such groups as nervous and spinal stimulants, emetics and cathartics would have served to connect together medicines of analogous properties, which are at present scattered apparently at random throughout the pages.

The author, indeed, has, to a limited extent, availed himself of this principle in treating of "Purgative Salts," and of "Anthelmintics," and he would have avoided the needless multiplication of isolated sections on individual drugs by a little further extension in the same direction. It is, we believe, also of great importance in a work on pure therapeutics to indicate, so far as possible, the connexion between the physiological and therapeutical action of drugs, and on these points Dr. Ringer's work is not as full as could be wished.

We can only feel disappointed, after perusing the long catalogue of ills which a favourite remedy, bromide of potassium, for example, is said to cure, without any attempt being made to illustrate its *modus operandi*, or any account given of its physiological action beyond the barest summary.

Passing by the unequal attention which is paid to important drugs, we notice that sometimes when several articles are placed under one head, no care is taken to differentiate their uses and modes of administration. Thus, after reading that the bromides of potassium, sodium, and ammonium, "in their action on the body exhibit considerable differences," we naturally look for the development of this general statement. The whole of the article is occupied with a detailed account of the fifteen or sixteen applications of bromide of potassium; not a line is devoted to its congeners bromide of sodium, or bromide of ammonium.

Professedly founded on Buchheim's work, it seems throughout as if Dr. Ringer was hampered by his German model, and it would perhaps have been wiser if he had either contented himself with a simple translation, or had boldly planned an original work, and done his best to bring it up to the present state of knowledge by *digesting* what is really known of practical and theoretical therapeutics. This, we admit, would be a task of great difficulty, demanding patient labour and a well-balanced judgment, and we regret that while we give Dr. Ringer all credit for the numerous useful hints that crop up so abundantly, and gladly acknowledge the information we have received, we cannot say that a perusal of his work has

done much towards systematizing or defining our previous ideas as to the real uses of medicines.

Indeed, a student would probably be bewildered by the want of arrangement of the long array of medicinal uses of drugs, and by the careless and inaccurate mode of expression, and would, in not a few cases, be actually misled by statements for which we cannot but hold Dr. Ringer responsible.

We do not care to enter on the unwelcome task of adding to the list of errors which have been already pointed out by others, but we hope that when Dr. Ringer is called upon to prepare a second edition, he will endeavour to repair the sins of omission and commission, and strive to render his treatise an accurate exponent of modern ideas, and a more perfect reflex of the present stage of therapeutics.

Within a few months Mr. Lescher has produced two compilations intended for the special instruction of Pharmaceutical students, but the book first named is now incorporated with the larger "Elements of Pharmacy." Though this work will, no doubt, be of value to students preparing for the examinations of the Pharmaceutical Society, who will follow the author through the subdivisions of his tables, we cannot recommend it as a safe "guide" to students generally.

No artificial tabulation of facts, however ingenious, could possibly compress within 200 pages any satisfactory information on such an array of subjects as are indicated on the title page. Under Section V.—Prescriptions; occupying but 16 pages, we find four subdivisions, viz:—(1.) The Latin language; (2.) The form of a prescription, with examples; (3.) Prescriptions, unusual or erroneous; and (4.) Posology. It is absurd to suppose that even the rudiments of Latin "Etymology," "Syntax," and "Idiomatic Construction," can be taught in eight pages, and we cannot put much faith in the author's Latinity when we read such a sentence as "*Unguenta ulceribus utilis*," which is rendered "the ointment, useful for ulcers;" and again, "*affricetur unguenta brachiis*," "let the ointment be rubbed into the arms;" while "afraid of fever" is Latinized "*Timidus febris*." Under the section Chemistry, within 50 pages, the author undertakes to guide a student to the salient points in (1.) Physics and the Laws of Chemistry; (2.) Simple Analysis; (3.) The Tests for the Purity of the Principal Chemicals; (4.) The Detection of Poisons; (5.) The Characters of the Inorganic Chemicals in the Pharmacopœia.

Surely, such an attempt carries with it its own condemnation, and we cannot help distrusting the author's chemistry when he states that iodide of potassium is incompatible with "acids or metallic salts," because "decomposition would ensue owing to instability of the compounds," and that tincture of perchloride of iron gives with mucilage of acacia "Black Iron Tannate." A really good text book of *moderate* size is an immense boon to the student, but the concentration of mental food, like that of medicines, can be pushed to an injudicious extent.

This little book will prove a useful guide to students and novices in the administration of anæsthetics.

Mr. Squarey prefers Clover's apparatus for the inhalation of chloroform, and the Clover face-piece adapted to a Cattlin's bag connected with an iron cylinder containing the compressed gas for the administration of nitrous oxide.

As a point in favour of nitrous oxide it is worthy of mention that up to the present time only two fatal cases from the exhibition of this gas have been recorded, one of which was accidental, owing to suffocation by the gag used, while in America the gas has been given in nearly 30,000 cases, without, as yet, a fatal case.

It is not easy to imagine what Mr. Clark's object was in publishing this pamphlet. He admits that he has never seen a mixture of sp. æth. nitr., vin. ipecac., and ant. tart. "used or prescribed," and informs his readers that the discovery of this novel combination has cost him "much thought, reading, and observation in this country, on the continent, and in the southern regions of the world."

There is every probability of a literal fulfilment of the author's concluding wish, that his "little history, like water, may find its own level."

WALTER G. SMITH.

A Manual of Instruction for Attendants on Sick and Wounded in War. By Staff Assistant-Surgeon A. MOFFITT. Published under the sanction of the "National Society for Aid to the Sick and Wounded." London: Griffin and Co., 10, Stationers' Hall Court. Pp. 113.

A BOOK written with the above philanthropic purpose should be leniently dealt with by the critic, but, with a view to the avoidance

of similar errors in any other manuals of the kind which may be brought out, we cannot avoid calling attention to the totally unsuitable character of much of the contents, of the waste of money incurred in publishing what cannot be of the slightest use to those for whom this book purports to be intended.

The work before us is entitled a manual of instruction for attendants on sick and wounded, or, in other words, for such as are to act as orderlies and nurses; and yet nearly one-fifth of it is devoted to "the anatomy of the human body," of which the following extracts are ordinary specimens, viz.:—*Osseous System*.—"Cranium, the skull. The cranium is made up of eight pieces, intimately united together, and forming a strong bony case for the protection of the brain. In front is the frontal bone; behind is the occipital bone; at the sides are the two temporal bones; above and at the sides are the parietal bones; and forming the base are the sphenoid and ethmoid bones."

"*The Muscles of the Body*.—The muscles constitute the flesh or lean part of meat. Muscles are divided into two classes, striped or unstriped—the former obey the will, and are called voluntary, the latter are not subject to the will, and are called involuntary;" and thus, with "the heart and blood vessels," and the nervous system, the latter of which commences with this information for the nurse, the nervous system is divisible into two chief portions, a centre and periphery, and then branches off into the cerebro-spinal and sympathetic system. As an offset to this depth of learning, however, a skeleton is given which may be of use; but, it would be more so were "radius and ulna" styled bones of the forearm; and "carpal bones" denominated bones of the wrist.

The chapters on "bandages and bandaging," "the dressing of sores, wounds, and injuries," and "the carriage of sick and wounded," are more to the purpose; but these too should be more definite and explicit, to afford correct useful information. Thus, speaking of lint, the author says, "it is a soft linen woven material, with a nap on one side," thus leaving the attendant to believe that the making of it, if wanting, is out of his power, whereas the best and most useful lint, because it is capable of being torn into thin strips, which the patent lint is not, is hand-made, produced merely by raising, with the blade of a dessert or dinner knife held vertically, a pile upon the surface of old linen stretched smoothly upon a very firmly and evenly stuffed pillow.

The directions given in a manual of this kind should, in our

opinion, be so explicit that the attendant can act under their guidance, and afford the wounded sufficient and real aid. "To secure fractures," we are told, that when bones of the extremities are broken, "splints of some kind should be employed to fix them, and prevent movement. If this be not done great suffering is caused to the patient during transport, and the injury may be so aggravated as to lessen considerably the chances of recovery. Two sets of splints, furnished with pads and straps might be made to answer as a temporary measure for the purpose, and should be carried attached to each stretcher." What the material is of which these splints are composed is not stated, but a foot note says "field transport splints," recommended by the author for use in the British army. In this style is much of the text written. Whilst pointing out the errors in this little book, there is still that in its pages which is valuable, and the directions as to the removal of wounded men, with the modes in which the bearers should conduct the transport so as to avoid the infliction of further injury during their carriage to the rear, are most useful, practical, and distinct. Had its whole contents been equally well given, it would have been a useful book; and by revision may still be made so.

Atlas of Ophthalmoscopy; representing the Normal and Pathological Conditions of the Fundus Oculi, as seen with the Ophthalmoscope; composed of 12 Chromo-lithographic Plates, containing 59 Figures drawn from Nature, and accompanied by an Explanatory Text. By Dr. R. LIEBREICH, the text translated by H. ROSBOROUGH SWANZY. Second Edition (enlarged and revised). London: John Churchill & Sons, New Burlington-street. Berlin: August Hirschwald, 68, Unter den Linden. Paris: Germer Baillière, Rue de l'Ecole de Médecine, 17. 1870.

THE original edition of this magnificent work is so well and so universally known that we need but draw attention to the alterations in this new edition now before us: the work is reduced in size, principally by curtailing the white margin of the plates, for, with the exception of Plate I., which has been reduced, all the plates are the same size as, and with the exception of Plates I., VI., IX., and XI., are identical with the originals. Plate I., representing the normal fundus oculi, has been coloured and the lining in it omitted—welcome alterations; Figures 2 and 3 on

Plate VI., have been judiciously replaced by an excellent figure of disseminate choroiditis; a very useful figure of hemorrhagic retinitis has been substituted for Figure 3, Plate IX. Figure 1, Plate XI., has been curtailed so as to make room for three new figures explanatory of changes in the optic disc consequent on intracranial causes, retinitis, and on retro-ocular affection of the nerve.

In addition to the reduction in size the author states in his preface that "considerable sacrifice has not been spared in order by reducing the price by nearly one-half to make this work accessible to a larger circle of practitioners and students."

The original edition was in parallel columns of German and French; the edition now before us is altogether in English, for which we are indebted to Mr. Swanzy, of Dublin, who has executed his portion of the work in a most able and creditable manner. Both as a scientific work and as a triumph of lithographic art this Atlas stands pre-eminent; any praise we could bestow on it would be feeble and fall far short of its merits; the circumstance of this splendid work emanating in part from a Dublin practitioner renders it doubly welcome to us.

1. *A Manual of Clinical Medicine and Physical Diagnosis.* By THOMAS HAWKES TANNER, M.D., F.L.S. The Second Edition. Revised and Enlarged by TILBURY FOX, M.D., Lond. London: Henry Renshaw. 1869. Fcap. 8vo, pp. 355.
2. *The Student's Guide to Medical Diagnosis.* By SAMUEL FENWICK, M.D., Assistant Physician to the London Hospital. London: John Churchill and Sons. 1869. Fcap. 8vo, pp. 176.
3. *Auscultation and Percussion; together with the other Methods of Physical Examination of the Chest.* By SAMUEL GEE, M.D., Fellow of the Royal College of Physicians, London; Assistant Physician to St. Bartholomew's Hospital. London: James Walton. 1870. Small 8vo, pp. 299.

DR. TANNER'S *Manual of Clinical Medicine and Physical Diagnosis* appears as a second edition, under the editorship of Dr. Tilbury Fox; it has undergone considerable enlargement, and contains much matter useful to the student and busy practitioner, and so arranged that they can easily find the information they seek.

Such a book as Dr. Fenwick's was needed, and we believe his

Students' Guide to Medical Diagnosis supplies the want; his method is admirably adapted for training the student to recognize disease. Take diseases of the stomach for instance; he gives first a short statement of the morbid anatomy of that organ, then of the physical signs which tell us about it, then he divides its disorders into acute and chronic; in connexion with the former he takes the symptom vomiting, and in connexion with the latter the presence or absence of pain, and he helps the student starting from these symptoms, to spell out the diagnosis. It is of the nature of such a work as this that it must leave many questions unnoticed, but it is eminently calculated to make a student observe for himself and think for himself, and if he once does so education in the true sense of the word has commenced.

We heartily commend Dr. Gee's *Auscultation and Percussion*; it is not a book which any man will care for except he wishes really to master the subject of which it treats, but those who do will find in it, devoid of all superfluity of words, a thorough exposition of the physical examination of the chest. Let us add, moreover, that the author has carefully given credit to the various honest workers, living and dead, to whose industry and genius the present generation owes its knowledge.

Quantitative Chemical Analysis. By DR. C. R. FRESENIUS. Fifth Edition. Edited by A. VACHER. London: John Churchill and Sons. 1869. 8vo, pp. 377.

Qualitative Chemical Analysis. By Dr. FRESENIUS. Seventh Edition. Edited by A. VACHER. London: Churchill and Sons. 1870. 8vo, pp. 264.

THESE books constitute the best guides to the study of analytical chemistry in the English language. The author is admittedly the most distinguished chemical analyst in the world—that is, so far as the *modus operandi* of the science is concerned. Both works have been almost wholly re-written; and, as compared with the last edition, they are greatly compressed, owing to an entirely new and more convenient arrangement of matter. The student will find the present form of these books very convenient; although the practising chemist may regret the elimination from the quantitative works of many of the special schemes of analyses which the former edition included.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By JAMES PAGET, F.R.S., D.C.L., Oxon; Sergeant-Surgeon Extraordinary to Her Majesty the Queen, &c., &c. Third Edition. Revised and Edited by WILLIAM TURNER, M.B., Lond.; Professor of Anatomy in the University of Edinburgh. London: Longman, Green, and Co. 1870. 8vo, pp. 850.

THIS edition of Mr. Paget's *Lectures on Surgical Pathology* has been revised by himself from the clinical point of view, and by Professor Turner from the pathological, and is little likely therefore to lose the position it has so long held as the best exposition we possess of surgical pathology. A man who enjoys among his professional brethren the unsurpassed reputation of Mr. Paget, for experience, sobriety of judgment, honesty and candour, is little likely to have time on his hands for other than clinical work, and he was fortunate in securing the co-operation of Professor Turner, whose practical acquaintance with minute anatomy, normal and pathological, is so well known.

Hand-Book of Physiology. By WILLIAM SENHOUSE KIRKES, M.D. Seventh Edition. Edited by W. MORRANT BAKER, F.R.C.S.; Lecturer on Physiology at St. Bartholomew's Hospital. London: James Walton. Small 8vo, pp. 838.

THE present edition of Kirkes's *Hand-Book of Physiology* has been called for so soon after the appearance of the previous one, that, as the editor informs us, there were not many important additions to our knowledge to be noticed; but the number of illustrations has been increased, and alterations have been made in the arrangement of the matter which render the work even better deserving than it formerly was of its position as *The Students' Physiology par excellence*.

The Baths and Wells of Europe, their Action and Uses; with Hints on Change of Air and Diet Cures. By JOHN MACPHERSON, M.D. London: Macmillan and Co. Small 8vo, pp. 336.

THIS is a little book, considering how extensive are the subjects of which it treats; marvellously little if we judge of their magnitude

by the ponderous volumes which our French and German confreres devote to their consideration, but it is an excellent little book; it contains all most of us wish to know about the baths and wells of Europe, and a certain amount of information respecting them is now-a-days necessary to every practitioner. We have read what Dr. Macpherson says of several of the springs with which we are personally acquainted, and we can vouch for the accuracy of his accounts of them.

Traité des Maladies des Yeux. Première Partie. Par X. GALEZOWSKI, Docteur en Médecine de la Faculté de Paris; Lauréate de la même Faculté; Professor Libre d'Ophthalmologie à l'École Pratique de la Faculté de Paris. Paris: J. B. Baillière et Fils. 1870. 8vo, pp. 464.

A Treatise on the Diseases of the Eye. First Part. By X. GALEZOWSKI, Doctor of Medicine of the Faculty of Paris, &c.

FEW, if any, of the several branches of medical science have outstripped or even equalled the marvellous advances achieved during the present century as regards the Diagnosis, Pathology, Etiology and Treatment of the various Diseases of the Eye; and as to the optical defects of that organ it may be safely stated that it is only within the last ten years that we have acquired anything like an accurate knowledge of the different anomalies of accommodation and refraction.

The growing interest exhibited in the study of Ophthalmology within the last few years has naturally led to an increased demand for works relating to the eye, and in consequence a proportionately increasing supply. Taken as a whole they bear a character of singular excellence, which the present work fully sustains. The discovery of the ophthalmoscope, no doubt, in a great measure accounts for the marked interest ophthalmology has excited during the last twenty years, and it promises fair to become universal as the various phenomena in the vast field of intra-ocular diseases become more connected and better understood.

The author of the treatise now lying before us is already well known as a writer on ophthalmology, especially in connexion with the diagnosis of cerebral disease, by means of the ophthalmoscope;^a

^a *Étude Ophthalmoscopique sur les Altérations du Nerf Optique et sur les Maladies cérébrales dont elles dépendent.* Par X. Galezowski. Paris, 1866.

the present work will doubtless add to an already well-earned reputation, and cannot fail to cause the enrolment of his name amongst those who are esteemed as the leaders and masters of modern ophthalmology.

We regret that time and space will only permit our taking a very hasty glance at the present work. We hope, however, to be able to point out some of the most striking and suggestive portions, and especially those which contain the opinions of the author upon subjects viewed from the standpoint of his own individual experience and research.

The work is divided into two parts, the first of which treats of the diseases of the eyelids, lacrymal passages, conjunctiva, cornea, sclerotic and crystalline lens, and is the subject of the present review. The second part, which is promised next year, will be devoted to the ophthalmoscope, diseases of the vitreous, optic nerve, retina, choroid, muscles of the eye, orbit, refraction and accommodation, forensic and hygienic medicine, and in addition an atlas of 20 chromolithographic plates. From this it will be seen that none of the subjects which ought to be included in a complete treatise on diseases of the eye have been neglected, and that due care has been bestowed on their proper order and arrangement. The work is admirably illustrated, if we except a few of the microscopical drawings.

What specially strikes us on a first glance at the work, is the large portion devoted to the description of the several diseases of the eyelids, and the great importance attached by the author to the affections of the lacrymal passages; an importance which cannot be overrated, more especially when we find those subjects so frequently treated in a cursory manner in works on diseases of the eye. Therefore we regard the author's oft-repeated injunction, as to the careful examination of the condition of the lacrymal passages when treating the various diseases of the conjunctiva, eyelids, cornea, &c., as one of great value, and demanding our serious attention.

The first five chapters, which are devoted to the consideration of the different diseases of the eyelids, are preceded by a short and succinct description of the anatomy, development, and physiology of the parts concerned. The same plan is adopted throughout the rest of the volume when treating of the diseases of the other parts of the eye; so that the whole serves not only as a ready book of reference, but also as a complete vade-mecum for the student and practitioner.

We shall now take a very hasty glance at some points which

strike us as possessing novelty and interest, as well as practical importance.

Blepharitis stands at the head of the list of diseases affecting the eyelids. The author recognizes two forms. 1. Furfuraceous blepharitis or pityriasis. 2. Glandular or bulbous blepharitis. In the first form it is merely the surface of the skin and points of exit of the cilia, which are affected; in the second it is either the sebaceous follicles or the hair bulbs which are attacked; in fact, this latter form generally follows as a natural consequence of the neglect of treatment of the first.

In this affection the lacrymal puncta are frequently found displaced, or constricted, and obstructions of the lacrymal canals very often produce blepharitis. Indeed so frequently is this the case, the author states, that of 150 cases of blepharitis which he has treated within the last two years, 60 were due to alterations in the lacrymal passages; he accordingly lays great stress on the importance of a careful examination into their condition, as part of the treatment of blepharitis.

The author's opinion as to the starting point of the inflammation which produces hordeolum is, that it is in one of the sebaceous glands, connected with the hair bulbs, and not in the subcutaneous cellular tissue, as is generally supposed. He grounds this opinion on the statement of Richet, as to the primary seat of furunculus being in the hair follicles.

Among tumours of the eyelids, we find a description of syphilitic gummous tumours, termed syphiloma by Ch. Robin. Those tumours are developed either in the subcutaneous cellular tissue, or in the orbicularis. Bouisson has found this affection in one of oculomotor muscles; but, up to the present, it has not been noticed as occurring in the eyelids. These tumours belong to the tertiary stage of syphilis. Dr. Galezowski gives two cases; one in which the tumour was situated at the internal part of the inferior eyelid, the other in which it was close to the lacrymal sac.

The treatment recommended is large and increasing doses of iodide of potassium, and the application of the tincture of iodine to the tumour.

In the article on trichiasis and distichiasis we find mention of three different operations for the radical cure of partial trichiasis.

1. The excision of a small fold of skin quite close to the border of the eyelid, as proposed by Desmarres père. 2. The plan adopted by the author, which consists in the extirpation of the hair follicles.

This is effected by introducing a fine chalazion hook between the inner and outer margins of the palpebral border, so as to include the portion containing the inverted cilia, which is then dissected out. The edges of the conjunctiva and skin being now brought together heal rapidly by first intention. 3. The operation proposed by Vacca Berlinghieri, which consists in dissecting up a flap containing the inverted cilia, to the extent of about two millimetres, each of the cilia being then excised and the flap replaced.

None of these methods appear to us equal in ingenuity and neatness to that recommended by Herzenstein;^a namely, the subcutaneous introduction of a silk thread between the cilia and the openings of the meibomian ducts; this is subsequently allowed to cut its way out. We have found this operation most efficacious in cases of partial distichiasis.

For general trichiasis an operation proposed by Anagnostakis is favourably spoken of; it is performed in the following manner: An excision is made parallel with, and about three millimetres from the palpebral border. The upward edge of the wound is now drawn upwards by an assistant, while the surgeon seizes with a forceps the muscular layer covering the tarsis, and excises it with a scissors. Three or four silk sutures are next introduced through the lower lip of the incision, then through the fibro-cellular layer covering the cartilage, in the place from which the muscular fibrils have been removed, and finally tied.

The operation of extirpation of the portion of the eyelid containing the faulty cilia, an operation termed "scalping" in this country, is one which in our opinion should be banished from surgery, save in most exceptionable cases. It is horribly disfiguring, and not alone that, but the cilia which are evidently intended as a natural protection against glare and dust being removed, the eye becomes inflamed and irritated, and the edges of the lids contracting and hardening (tylosis) rub against the cornea and produce a condition of chronic vascular pannus. We do not make these remarks unadvisedly, but as the result of our own personal observation of these cases, and we are glad to find our opinion endorsed by Wecker.

Arlt's transplantation operation and Graefe's modification are duly noticed.

In the several operations for ectropion the author states that temporary occlusion of the eyelids (blepharorrhaphic) after an

^a Archiv. für Ophthalmologie: Bd. xii., p. 76.

operation, is considered by Mirault, Nélaton, and Richet as essential to its success.

One of the most interesting and remarkable sections in the work is that devoted to the diseases of the lacrymal passages. Dr. Galezowski adopts the theory of Bérard, elaborated by Richet, as to the manner in which the mechanism of excretion of the tears is carried on.

“According to Richet, he says, the valve with which the inferior extremity of the nasal canal is provided, allows a free passage to liquids in the downward direction, but prevents the introduction of air or liquids from below. In fact every one is aware that when they make the effort to blow their nose, air easily enters the cavity of the tympanum, but does not punctrate into the lacrymal sac.

“When the walls of the sac are separated by the action of the anterior lacrymal muscle, there is a tendency to the formation of a vacuum in its cavity, and the nasal canal is firmly closed; the tears will then be drawn through the lacrymal puncta.”

In addition to this Dr. Galezowski conceives that the act of respiration tends to produce a vacuum in the nasal canal, which, acting as a second force, draws away the tears accumulated in the lacrymal sac, and not from the surface of the conjunctiva, as Sédillot supposed, into its channel. In this manner the sac is emptied, and the tears flow through the nasal canal into the nasal fossa.

This theory, however ingenious and plausible, does not, we think, stand the test of a very critical examination. Time forbids our entering on the question now, but to those who are interested in the subject we would recommend the perusal of a very remarkable letter, addressed by Dr. Giraud-Tenlon to Dr. Delgado Jugo, (Madrid), originally published in a Spanish journal, *El Pabellon Medico*, and subsequently in *Les Annales d'Oculistique*,^a in which this subject is treated in a masterly manner, and the ingenious theory advanced, that the mechanism of the excretion of the tears is effected by capillary attraction. We are surprised to find no allusion to this letter in the present work.

Dr. Galezowski has evidently bestowed special attention to the various diseases and affections of the lacrymal passages, and the result is, that the portion of the work devoted to them, forms a most valuable treatise on this too often neglected subject.

He speaks strongly in favour of Bowman's operation of slitting

^a *Annales d'Oculistique*. T. lxii. 5-6. Nov. et Dec. 1869.

up the canaliculi, and the subsequent introduction of the probe into the nasal canal, in cases of obstruction, inflammation, catarrh, &c., of the sac and canal. We find some very practical remarks on catheterism of the nasal canal. "In cases," he says, "where false passages and blind fistulæ, together with caries of the bones exist, it becomes necessary to allow a sound of small calibre to remain in for some time. For this purpose I have had small sounds made in the form of a crook (*sondes à crosse*), which I leave in for twenty-four or forty-eight hours. Every day I remove the sound, clean it, and reintroduce it. In this manner the canal is quickly re-established and each new introduction is accomplished without difficulty."

An article headed lacrymal conjunctivitis gives an account of a form of conjunctivitis first described by the author in two papers published in the *Gazette des Hôpitaux*,^a a translation of which also appeared in this journal.^b

Lacrymal conjunctivitis is simply an irritation and inflammation of the conjunctiva, produced by the retention of the tears in the conjunctival sac, owing to some obstruction of the lacrymal or nasal canals. The symptoms are generally well marked, though frequently mistaken for those belonging to other affections. For instance, it is not at all uncommon to see this form of conjunctivitis set down as due to granular lids; the mistake arising from the appearance presented by the engorged follicular and acinous glands.

"I have, more than once," says Dr. Galezowski, "seen very eminent ophthalmologists mistake these engorged glands for granulations, and aggravate the malady by repeated cauterizations."

A striking example of this came under our own observation a short time ago. The patient, after six months' attendance on an ophthalmic surgeon, who daily cauterized and scarified the lids, which resulted in the production of well-marked ptosis of the affected eye, was quite restored in a few weeks, the treatment being chiefly directed to the curing of an obstruction of the lacrymal passages.

The article on granular conjunctivitis is replete with interest. The author adopts a threefold division of granulations, based on the elements which compose them; he divides them into,

^a Essai sur la conjunctivite lacrymale, &c. (*Gazette des Hôpitaux*, 1868, p. 433).

^b Lacrymal conjunctivitis (*The Dublin Quarterly Journal of Medical Science*, Nov., 1869, p. 675).

1, papillary granulations; 2, vesicular granulations; 3, follicular granulations. This division resembles that adopted by Stellwag von Carion, except that instead of vesicular granulations he recognized a form which he terms mixed trachoma. This Dr. Galezowski rejects on the ground that it does not represent a distinct variety.

He also rejects the theory that the granulations are a neo-plastic formation similar to tubercle; but on the other hand he regards with more favour the hypothesis of their parasitic origin. He is firmly convinced as to their contagiousness. "In my opinion," he says, "it is now amply demonstrated that direct contagion is the most frequent, I might even say the only cause of the appearance of the malady.

"Conjunctival granulations are, in fact, most contagious, and in order to contract granular ophthalmia it merely suffices to wipe the eyes with a piece of linen, or to wash them with water, previously made use of by a person infected.

"The inoculation of blennorrhagic or leucorrhœic matter may also produce granulations, and frequently soiled linen impregnated with this secretion easily transmits this affection to the eyes. It is thus, we may explain the frequent occurrence of granular lids amongst washerwomen. I have remarked that more than a third of the patients attending my clinique for granular lids, are engaged in this employment. It is accounted for by the fact that they wipe their eyes with the soiled linen, on which there are, frequently, spots of blennorrhagic matter, and it is thus the matter inoculates the eyes and produces either acute ophthalmia, or chronic granulations."

In speaking of the treatment special attention is drawn to the importance of examining the state of the lacrymal passages.

"I have often," says the author, "effected a marked improvement by merely treating the obstructed and inflamed lacrymal passages."

In the treatment of the different forms of keratitis, especially phlyctenular and suppurative keratitis, the insufflation of calomel and the application of the yellow oxide of mercury, as soon as the first inflammatory symptoms have subsided, are highly recommended. It is a matter of surprise to us that they are not more frequently employed in this country, we are so convinced of their efficacy. In suppurative keratitis, when the inflammation and redness are very considerable, Dr. Galezowski states he is in the habit of prescribing the alternate instillation of solutions of Calabar bean and atropine. "The first of these," he says, "dilates the vessels, the second contracts them. In virtue of the combined action of both these agents, the vascularization diminishes."

The same treatment is recommended in cases of sclerotitis.

Though adopting a very extensive classification of the various forms of iritis, the author tacitly admits the impossibility of diagnosing, with any degree of accuracy, the forms termed blennorrhagic, gouty, rheumatic, &c. He points out, however, certain peculiarities which may, possibly, prove of some assistance in forming a correct diagnosis.

In the last chapter which is devoted to the treatment of cataract, the author speaks most favourably of von Graefe's linear peripheral extraction; by the bye, we are surprised to find it designated by the original title "modified" (*modifiée*), which, as is well known, the late lamented author abandoned more than a year ago.^a Dr. Galezowski sums up the results he has obtained from this operation as follows:—

"Of thirty-five cases, twenty-six have been completely successful, either from the first, or after an operation for secondary cataract. One eye was totally destroyed by suppuration; and two by irido-choroiditis; in four cases iritis or irido-choroiditis occurred, but without serious consequences; the sight, in fact, may be restored, and in one of these cases I performed a second iridectomy."

The author describes an operation he has introduced, and which he terms, *méthode combinée*; it consists first of all in one, or several lacerations of the anterior capsuli, the object being to produce a softening of the lens substance. As soon as this is effected he performs either a simple or combined linear extraction.^b The cases for which he considers this modified operation suited, and the conditions most favourable for its performance, are summed up as follows:—

"1. Cataracts formed by opacity of the antero-posterior cortical layers, the nucleus remaining transparent.

"2. Soft, incomplete, cataracts, without a nucleus, and either in process of formation or arrested in their evolution.

"3. This operation should not be performed on persons over forty or forty-five years of age. Beyond this age the nucleus becomes too dense to undergo softening.

"4. Traumatic cataracts which do not contain a foreign body; otherwise the latter may be displaced during the laceration, and lead to serious consequences.

^a Vide Archiv. für Ophthalmologie. B. xiv. 3, 1863.

^b If we do not mistake Soelberg Wells mentions a similar mode of operation as having been successfully practised and much advocated by Bowman.

"5. Adherent cataracts, accompanied by iritis or irido-choroiditis should not be operated on by this method.

"6. The corneal incision should neither be too near, nor too far from the scleral border. It should generally be made about three millimetres from the external border."

Dr. Galezowski concludes with some general considerations on the operation for cataract and the method to be chosen. The volume closes with an article on dislocation of the lens.

In conclusion, we heartily congratulate Dr. Galezowski on the genuine success of his undertaking, and we cordially recommend his work to the attention of our confrères, feeling confident that they will find in it all the requirements of a practical treatise on diseases of the eye.

Guide to the Royal Zoological Gardens, Phoenix Park. By Dr.

CHARLES A. CAMERON, Prof. in Royal College of Surgeons, &c.

Sixth Edition. Dublin: Robert C. Gerrard. 1870. Pp. 47.

As this Guide is intended for ordinary visitors to the gardens, scientific phraseology is avoided as far as possible; reference is, however, briefly made to the structure of the various families, and to the principles of classification, and the work is altogether written in such a way as is eminently calculated to help those who use it intelligently to enjoy what they see, and to produce in them a taste for natural history.

WORKS ON PHTHISIS, AND ON CHANGE OF
CLIMATE IN ITS TREATMENT.

1. *Clinical Lectures on Pulmonary Consumption.* By FELIX VON NIEMEYER, M.D., Professor of Clinical Medicine in the University of Tübingen. Translated by J. C. BÆUMLER, M.D. New Sydenham Society.
2. *The Published Writings of the late THOMAS ADDISON, M.D.* Edited by Dr. WILKS and Dr. DALDY. Article IV.—On the Pathology of Phthisis. New Sydenham Society.
3. *St. George's Hospital Reports.* Vol. IV., Art. X.—The Causes of Pulmonary Consumption. By C. THEODORE WILLIAMS, M.D.
4. *Fondements et Organisation de la Climatologie Médicale.* Par M. le Docteur ED. CARRIÈRE, Lauréat de l'Institut. Paris: J. B. Baillière et Fils.

5. *The Climatic Treatment of Consumption and Chronic Lung Diseases.* By JOHN C. THOROWGOOD, M.D., Lond. London: H. K. Lewis.
6. *Winter and Spring on the Shores of the Mediterranean.* By J. HENRY BENNET, M.D. Fourth Edition. London: John Churchill and Sons.
7. *The Climate of the South of France.* By CHARLES THEODORE WILLIAMS, M.A., M.D., Oxon. Second Edition. London: Longmans, Green & Co.
8. *Mentone and San Remo.* By EDWIN LEE, M.D. Second Edition. London: W. J. Adams.
9. *De l'Influence du Climat d'Arcachon dans quelques Maladies de la Poitrine.* Par le Docteur G. HAMEAU, Bordeaux.
10. *A Season at St. Moritz.* By J. BURNEY YEO, M.B., Lond. London: Longmans, Green & Co.
11. *Medico-Chirurgical Transactions.* Vol. LII., Art. XII.—On the Treatment of Phthisis by Prolonged Residence in Elevated Regions. By HERMANN WEBER, M.D.
12. *Egypt and the Nile considered as a Winter Resort for Pulmonary and other Invalids.* By JOHN PATTERSON, M.D., Egyptian Medical Service. London: John Churchill and Sons.
13. *The Climate and Resources of Madeira, as regarding chiefly the Necessities of Consumption and the Welfare of Invalids.* By M. C. GRABHAM, M.D.; &c. London: John Churchill & Sons.

At this season many a physician is called upon to decide whether some patient, suffering from pulmonory consumption, will winter at his home or will seek a residence elsewhere, during the cold months. On the propriety of such a measure, and on the whole subject of change of climate for the phthysical, there is considerable difference of opinion. On the one hand it is easy to point to young persons who presented the symptoms from which experience has taught us to predict an unfavourable termination, but who, nevertheless, after a change of climate, have been restored to apparent health; and, on the other, there are not a few families whose members, as they grow up, have, one after another, fallen victims to consumption—in which one hears how those of them who were carried, at great cost and inconvenience, to some distant abode, succumbed to the disease much more rapidly than those who

remained among the friends and comforts of home. Three members of a family, of ample means, in the North of Ireland, were, one after another, attacked with phthisis, the first was carried to the Nile and lived from the commencement of the disease eight months, the second sought refuge at Malaga and lived two years, the third remained at home and lived eight years. The measure is therefore one which no physician will lightly recommend, and the great number of works which have lately reached us, whose object is to aid him in arriving at a decision, leads us to a consideration of the entire subject. First, for notice, we have placed the translation, which has just appeared, of Professor Niemeyer's Lectures, one of the many valuable continental works for the possession of which in English we are indebted to the New Sydenham Society. The views of this distinguished physician have gained a very considerable acceptance with some of those most competent to judge of their correctness, and we refer to them now because we are convinced that his observations will enable us to understand why, in some cases of consumption, benefit, and in others only injury, has resulted from a change of climate. A complete account of the recent researches on the pathology of phthisis will be found in Professor Cuming's last report on medicine in this journal, and a lucid exposition of the generally received doctrine as to the nature of tubercle, also written by him, in the number for August, 1868. We will, therefore, on the present occasion, allude but briefly to the pathological part of the subject. The term tubercle, as now used by most advanced pathologists, is restricted to the hard, homogeneous, gray, or vitreous-looking bodies, in size seldom larger, and often smaller, than number four shot, which are found on the membranes of the brain, on the serous covering of the intestines, scattered through the lungs of *a certain proportion* of consumptive persons, and in various parts of the body, in the disease known as acute tuberculosis.* With the object of determining the mode of origin and development and the true nature of these tubercles, many laborious investigations have been undertaken, an account of which will be found in Professor Cuming's papers. What concerns us now is that while the presence of these tubercles unquestionably indicates a wide-spread contamination of the system, against which, in the present state of our art, we cannot hope to contend successfully, it is an error to suppose that they are present in the early

* Modern doctrine does not deny that these bodies may become cheesy, soft, and yellow, but so may many other morbid structures during their decay.

stages of any but a few exceptional cases of pulmonary consumption. It has long been known that in a certain proportion of those who, with condensations and cavities within their lungs, wasted and sweated, and were called cases of phthisis, the morbid change was really set a going by the inhalation of particles of fine dust, by the retention within the air cells of extravasated blood, by fibroid or syphilitic deposits, or occurred as the sequel of pneumonia; but in the vast majority the process was looked upon as the result of tubercular growths, an opinion which caused physicians to undertake, with little hope, the management of such cases, and more particularly to attach but a secondary importance to treatment directed against inflammatory processes in the lungs, except where these were severe and gave rise to distressing symptoms; according to Niemeyer, however, inflammation, characterized rather by persistence, and, if subdued, by liability to return, than by intensity, is the process to which, in the great majority of cases of consumption, the condensation and excavation are due; and this inflammation is not, as a rule, dependent on the irritation of tubercles, but has been initiated by exposure to cold, and has in most cases as a necessary condition to its existence, a certain vulnerability or unhealthiness of the system—an unhealthiness which may be congenital or acquired, and which, if congenital, is intensified by, and if acquired, has originated in influences which “hinder or disturb the normal development and maintenance of the organism.” If, as Niemeyer maintains, the majority of cases of phthisis arise in this manner, it is evident that with the exception of such treatment as the accidents and complications of the disease may require, the remedial measures which are good in consumption will form themselves into two groups, those which subdue the existing inflammation, or, when once it has been subdued, protect the patient against its return, and those which build up and strengthen the system. The value of change of climate, the subject now more immediately under consideration, lies in this, that it may be made to fulfil both indications. Have tubercles, then, no connexion with consumption? They have, according to Niemeyer, no necessary or invariable connexion. In the bodies of many phthisical persons they are not present at all, but in a certain number they are, and when they are, there are two ways in which they may be related to the phthisical change, that is to the cheesy formations, the induration and the excavation of the pulmonary parenchyma, which constitute the *invariable* anatomical alterations in consumption.

In a few exceptional cases, they are present from the outset. The induration and excavation are the results of an inflammation set a-going by the tubercles. In the great majority, however, they are secondary, a consequence of the cheesy formations. For an account of the way in which cheesy deposits may determine an eruption of tubercles, our readers must refer to the papers already mentioned, or to Niemeyer's work.

In relation, however, to the subject now before us, the rôle of change of climate in the treatment of pulmonary consumption, the clinical aspect of the question assumes the utmost importance, for in cases of tuberculous consumption, whether they were tuberculous from the outset or have become so, hope of benefit from change of climate is out of the question, and to our minds the most valuable passages in Niemeyer's Lectures are those in which he treats of the diagnosis of tuberculous as distinguished from ordinary phthisis.

"The development of tubercles in phthisical lungs may take place in so latent a manner that it cannot be diagnosed, or, at all events, not with absolute certainty. In many other cases, on the contrary, especially in those in which the lungs become the seat of very numerous tubercles, and in which the tuberculosis extends to the other organs also, the diagnosis does not present any difficulty. When we find a patient who is suffering from pulmonary phthisis becoming very short of breath, without any extension of the dulness over his thorax; when the pyrexia continues, in spite of the most careful treatment, and when the remittent fever becomes a continuous one; when diarrhœa takes the place of a tendency to constipation which may have existed before; when to the other symptoms hoarseness or aphonia, or the well-known signs of an affection of the membranes at the base of the brain, supervene, then we may assume with perfect confidence that in the case before us a tuberculosis has associated itself with the phthisis. The cerebral symptoms in the young, in whom tuberculosis has a special tendency to attack the membranes of the brain, and in older persons the symptoms on the part of the intestines and the larynx, furnish the chief points for the diagnosis.

"That form, lastly, under which a *primary tubercular phthisis* commences and takes its course, is essentially different from those hitherto considered, and is mostly so characteristic that the diagnosis of this not very frequent form of phthisis is, as a rule, easy. In the first place the prodromal catarrh is absent. The pyrexia and the 'consumption' do not begin only at the time when the patients expectorate profuse muco-purulent sputa, but, on the contrary, the eruption of tubercles, especially if very intense, occurs with a considerable elevation

of temperature, and with a rapid consumption of the body by a high pyrexia. If we hear from a patient that he has only commenced to cough and to expectorate, after having for weeks past rapidly become feeble, pale and thin, we must suspect him to suffer from a tubercular phthisis. This suspicion gains further ground if the patient is unusually short-breathed, and if the physical examination of the chest yields at first negative results. Later on the percussion sound may, by subsequent pneumonic processes, become dull, the respiratory murmur bronchial, the *râles* consonant, but in some cases only do the infiltrations of the lungs become so extensive as in those forms of pulmonary phthisis previously considered. At an early period the tone of the voice and of the cough generally becomes hoarse, and when the tubercular affection of the larynx is considerable and spreads rapidly, the well-known painful symptoms of laryngeal phthisis come on. The signs of intestinal tuberculosis, and of intestinal tubercular phthisis, are not as a rule, long in making their appearance. The consumption is increased by abundant diarrhœa, the abdomen becomes tender on pressure, &c. The disease rarely lasts more than a few months. Most of the patients succumb even at an earlier period."

"A tedious and troublesome cough with but little expectoration, which, not containing much formed material, corresponds to the 'sputum crutum' of the ancient, and to the 'purely mucous sputum' of the more modern authors, is most suspicious. *Cæteris paribus*, there is reason to fear that we have to deal, not with a pneumonic process, but with a *tuberculosis* of the bronchial mucous membrane and of the alveoli; and we can confirm every word of *Canstatt's*, 'that it is a very critical symptom, and one which strongly arouses the suspicion of a tuberculosis, when obstinate cough and pyrexia are accompanied by sputa which for a long time keep the crude character, like those of acute bronchitis.'

Second on our list we have placed Dr. Addison's paper on the Pathology of Phthisis. We do so in justice to his memory, and because his faithful descriptions will amply repay the perusal, even of those who are familiar with more recent contributions to our knowledge. At a time when physicians were possessed with the idea that phthisis was invariably tubercular, he persistently enunciated views very similar to those of Niemeyer. Reproached for devoting himself to pathological investigations to the exclusion of more practical studies, he was really making discoveries which, if they had been properly appreciated, would have helped to place the treatment of phthisis on a rational basis.

Excluding, then, altogether from consideration tuberculous cases, we come to the more immediate subject of our present review—the

object of change of climate in consumption, the influence it may be expected to exert, and the rules to be observed in recommending it. Those who hold the most opposite views as to the pathology of the disease, as well as those who have never troubled themselves much about its nature, but merely observed it at the bed-side, agree in the opinion that the more phthisical patients eat, and the better they digest their food, the greater prospect is there of an indefinite postponement of death, and even of something which, except for the permanent destruction of a portion of lung, may be considered recovery. Further, most of us know that, setting aside some very exceptionally constituted individuals, who are able to get on wonderfully well without leaving their house for months at a time, the great majority of people, and above all, of people at the age when phthisis is most common, cannot eat, nor digest, nor thrive, without daily exercise in the fresh air; at the same time we know that the diseased lungs of the consumptive are so sensitive that, during much of our winter, they must, in most parts of Great Britain and Ireland, remain cooped up inside their houses. We want, then, to find a place where the climate is so mild that, for several hours on most days during the winter, they may, without risk, go into the fresh air. Side by side with this, however, let us put another fact which few who have seen anything of consumption can doubt, namely, that those who are suffering from that complaint, when we have hot weather in summer and autumn, and, above all, if we have hot and moist weather, go down the hill fast—their cough may not be so troublesome as in winter, but they eat less and digest with greater difficulty, and grow weaker and thinner, and sweat more, and sink towards the grave with infinitely greater rapidity than in the cold season. The problem, then, in each individual case is to find a winter residence as dry and as cold as the lungs will bear. Whenever an extreme susceptibility of the diseased organ necessitates us to select a locality where there is much heat and moisture we are making a movement in which the chances are we will soon be out-flanked, and from this position our patient must move as soon as we have secured reasonable alleviation of the local distress. This conclusion, to which a consideration of the phenomena of pulmonary consumption naturally leads, is certainly supported by experience, without which, in any therapeutic question, *à priori* conclusions are not worth much. Dr. Thorowgood devotes a considerable portion of his very useful volume to this point, arguing that since consumption is common in very warm places, and

specially rife where the soil is damp, heat and moisture are not (as some seem to think) to be desired in a winter resort for the phthisical. Three years ago, in our notice of Dr. Fuller's excellent work on *Disease of the Lungs*, we drew attention to this subject, and those who have the largest practice among persons whose circumstances enable them to try various climates are, we think, pretty unanimous in their condemnation of some of the health resorts which formerly were commended. "My experience," said Dr. Gull to a patient about whom Dr. Weber consulted him, "with regard to the warmer health resorts is great; but it is, unfortunately, not favourable."^a With this general principle, then, we set out—when a consumptive leaves his ordinary residence in this country to seek, either in a more sheltered situation in Britain or abroad, a better place in which to winter, he should not seek a warmer one than his lung absolutely needs. Beyond this the problem, for its solution, requires a study of details, and these details naturally arrange themselves into two groups—those connected with the sick man whose case is under consideration, and those connected with the localities among which our selection is to lie.

First, as concerns the invalid, the completeness of the remission in the course of the disease is probably the most important point. In few cases of consumption does the disease keep steadily going on, and in those in which it does, change is hardly to be thought of. It is during a lull in the progress of the malady that change is to be sought, and the more perfect the lull, the more thoroughly inflammatory action has ceased, the more perfect the disappearance of the evening fever, the less is the danger of injury, the greater is the hope of benefit from quitting home, and the more freedom may the physician exercise in the choice of a locality. Next in importance is the degree of susceptibility to intercurrent inflammations. Some phthisical patients have no great tendency to catch cold, while others, on the least exposure, get a localized bronchitis, or pleurisy, or pneumonia; the former may go to the comparatively cold and dry, and on that account more valuable, health resorts; the latter must seek those which are warm, and in which the air is moderately moist. There is, too, in some an extreme irritability of the bronchial tract, which causes distressing cough, occurring with a frequency and severity out of all proportion to the amount of expectoration to be brought up, and often ending in

^a The most rapid phthisical disorganization of the lung we ever saw occurred in a gentleman who arrived in Calcutta just as excavation was commencing in one apex.

the loss, through vomiting, of the food which has been taken, to such persons, also, the warmer and moister climates are necessary.

Then, the sort of gastric disturbance present is to be considered, that which is indicated by a pale and slightly furred tongue, want of appetite, and feeling of distressing fulness after food, will be benefitted by the climates we have put towards the top of the list, page 364, while that kind of dyspepsia in which the tongue is red, and there is thirst and pain after food, will oblige us to select climates such as those on page 365.

To the condition of the nervous system some French and English writers attach considerable weight in determining their selection of a winter residence for the consumptive. We cannot say that we have ourselves noticed among the phthisical any marked tendency either to an unusually mobile or unusually torpid habit in the nervous centres, and we are inclined to think that in the greater or less liability to the symptoms we have enumerated will be found more definite indications for our guidance. It is, however, true that the air of such places as Nice has a tendency to render those accustomed to our moister climate sleepless, and where we have to do with persons who have naturally a restlessness and want of composure about them, this circumstance would lead us to send them in preference to Hyeres or to Pau. Likewise must we take into consideration the habits and tastes of the sick person, and the amount he can without serious inconvenience spend, for we may rest well assured that a consumptive will not thrive where he has nothing to interest and amuse him, nor be benefitted by a change which costs so much as to embarrass him.

Secondly, we have to consider a few details about the localities, and about them our most useful information is derived from the experience of their effects on those who have tried them. The effect upon the appetite is the first thing to be ascertained. Practically the answers we receive on this point inform us whether the invalid was able on most days to be in the open air without suffering an aggravation of his cough. If, in addition to a favourable report in this respect, we ascertain that the place afforded such a dietary as the consumptive should have, and if we discover that those who were not specially fortunate in their companions, nor in having "resources within themselves," did not nevertheless find time hang heavily on their hands, we may conclude that the locality under consideration is to be recommended to consumptive persons whose disease assumes the character of our experimental case.

A Selection of Winter Residences for the Phthisical. Mean Winter Temperature of Dublin, 42° Fahr.

Winter Residences more or less suitable for the Consumptive in proportion as the local disease is stationary and fever absent but appetite wanting, digestion imperfect and strength feeble.

Mean Winter Temperature	In United Kingdom	Abroad	Observations
13.47	...	Montreal,	In virtue of its extreme dryness and comparative calmness, the air of Eastern Canada suits admirably consumptive persons, presenting typically the conditions indicated in the margin during December, January, February, and March, after which they must leave before thaw commences.
39.91	Clifton,	Norway,	Suitable under same conditions as Montreal.
40.00	St. Leonards,	...	
40.60	Cheltenham,	...	
	Dundrum,	...	
	Ballybrack,	...	
	Sutton,	...	
41.89	Ventnor,	...	
44.1	Queenstown,	...	Affording excellent accommodation, presenting abundant objects of interest to the invalid, and having a higher temperature than the English towns in the same class, Queenstown is highly to be commended.
	Summercove,	...	A little village within Kinsale Bay; invalids who have wintered at Queenstown may try it for a short time in spring, when Queenstown by reason of its exposure to the East is unsafe; apartments sufficiently good to satisfy many could be obtained; it is open, however, to two great objections, there is nothing to interest an invalid and the walks (within the sheltered area) are too limited.
	...	Montreux,	For consumptives who require only shelter and air a little warmer than at home, Montreux, on the Lake of Geneva, can be confidently recommended; accommodation during winter is good and not costly; there is sufficient amusement, and during the summer they may go to Chateau d'Oex, which is 2,900 feet above the sea.
47.8	...	Nice,	Unsafe for most consumptives on account of its windiness and liability to sudden changes of temperature.
48	...	Cannes,	
48.5	...	Mentone,	
49.1	...	San Remo,	
56	...	Malaga,	The climate of Malaga is admirable, but the food and accommodation so bad that at present it is not to be recommended.
62	...	Cairo,	

A Selection of Winter Residences for the Phthisical. Mean Winter Temperature of Dublin, 42° Fahr.—Continued.

Winter Residences more or less necessary for the Consumptive in proportion to the incompleteness of the lull and the liability to irritative cough, hemoptysis, and intercurrent inflammations.

Mean Winter Temperature	In United Kingdom	Abroad	Observations
	Rostrevor,	...	Beautifully situated, sheltered, affording good accommodation, and sufficient variety of walks and drives, Rostrevor may occasionally be selected for a winter residence, but it is not dry nor warm enough for most consumptives.
40·00	Mallow,		} Relaxing.
42·38	Hastings,	...	
44·00	Bournemouth,	...	
44·00	Torquay,	...	
	Penzance,	...	
	...	Biarritz,	Excellent accommodation, which during winter is comparatively cheap.
42·8	...	Pau,	Affords abundant means of recreation. Special feature stillness of air.
47·3	...	Arcachon,	
53·1	...	Hyerès,	
		Palermo,	Probably one of the best climates for cases of the type described in margin; recent accounts speak of accommodation as being very good.
55·0	...	Algiers,	
72·50	...	Cape of Good Hope,	December, January, and February are the summer months at the Cape, and invalids whose chest symptoms are troublesome except when they are breathing a very warm air, will find Wynberg or one of the other suburbs of Cape Town less relaxing than some places whose temperature is lower, for which reason we place the Cape above Madeira and Alexandria.
60·60	...	Madeira,	
62·00	...	Alexandria,	The mean annual temperature of Alexandria is below that of Cairo, but during the months of November, December, and January there seems little difference in that respect; in Cairo, however, the air is very dry, whereas in Alexandria it is loaded with moisture.

This mode of investigating the subject is not imposing, but the information it will afford us is of infinitely greater value than that derived from a study of tables of mean temperature and barometric readings. To it we must be careful to add a knowledge of the general healthiness or unhealthiness of the place. On the continent of Europe we should ever be on our guard against towns where enteric fever prevails; where it is endemic new arrivals are specially obnoxious to its attack, and many are the victims which it finds among the consumptive and their travelling companions.^a In cases in which the disease is at all advanced, it of course becomes a matter of the first importance that the winter residence we recommend should be comparatively easy of access, and of hardly less moment is it that there should be within a reasonable distance some cooler spot to which the invalid can be carried when the summer heat begins. We have known this want painfully felt at Arcachon, where even the summer town, as the part next the sea is called, is much too hot for a consumptive during July and August.

In the table, pages 364-5, we have arranged a number of the more celebrated winter residences abroad and some of those at home, which, though less renowned, are of more importance to us from their greater accessibility, and we have placed them according to the principle that those which, by virtue of their comparative coldness and dryness,^b are truly invigorating, should be at the top of the list; while those which, by virtue of their comparative warmth and moisture, are soothing and necessarily more or less relaxing, should go to the bottom. Where we should send any particular case will depend on a consideration of the individual peculiarities already mentioned, but *cæteris paribus*, the higher in the list we can keep the greater the prospect of permanent benefit.

Those who wish information about Madeira will find what they want, together with a good many disparaging reflections on Mentone, in Dr. Grabham's *Climate and Resources of Madeira*. We have no doubt his account of the history, the social aspect, and the meteorology is correct, and we believe that some cases of chronic laryngeal and bronchial affections would find the air particularly soothing, but in his estimate of the general hygienic advantages of

^a We feel bound specially to protest against permitting persons to go to Naples during the decade (15 to 25), when they are most likely to catch enteric fever.

^b The degree of coldness of the air which consumptive lungs will bear depends on its purity and stillness; thus a temperature which would be hazardous at Nice would be perfectly safe at Pau.

the island, and specially in his recommendation of it for the phthical, we cannot concur. Against us we have no mean authorities, Clark and Chambers (whose assertion, when he treats of climates for the consumptive, "Madeira is the best," Dr. Graham has with singular negligence omitted to quote), but in support of us, to refer to no other experiences, we have that of the Brompton twenty, in itself almost conclusive.

The claims of Egypt as a health resort have recently been the subject of controversy between Dr. Henry Bennett and Dr. Robinson, of this city. The position accorded to Lower Egypt (Alexandria and the Delta) in our table, most of those who understand the subject would, we think, readily pronounce the proper one, and much as life on the Nile or at Thebes has been praised, we have difficulty in believing that any physician who had ever been there could think of recommending such a residence; to a few people in this land the costliness of the enterprise would be a matter of no moment, and if a consumptive, who belonged to this class, had the protection of a friend who was a match for dragomen skilled in all the ways, Asiatic and European, by which travellers can be victimized, he might secure an excellent table on the Nile and he could bring a physician with him, but he could only protect himself from changes of temperature, which are sudden and great, by shutting himself up in his cabin, and, in no place with which we are acquainted is home-sickness more likely to overtake a man than during the monotony of a Dahabiah journey, he would, of course, have an opportunity every few days of seeing temples and tombs unequalled in antiquity and in magnitude, but he must generally expose himself to a powerful sun to reach them, and to the chilliness of a vault, if he enter them, and, after all, most of our patients belong to that large class of human beings for whom the comparatively common place sights and incidents of a European watering-place would possess greater attraction than the monuments of a by-gone greatness.

We are not disposed, however, to give an unfavourable opinion of life in Cairo itself.^a The sea voyage to Alexandria in one of the

^a Some months ago Mrs. Appleton called upon us; this lady purposes establishing a Sanatorium at Cairo, and if she is heartily supported by even a few of the physicians whose names are affixed to her circular, her Sanatorium will not want occupants. At the time she was in this country she was unable to say the part of Cairo in which the apartments she had secured were situated; if she has chosen a suitable position, and brings to the management of her enterprise energy and tact it should succeed, and we hope, for the sake of invalids, it will.

magnificent steamers of the Peninsular and Oriental Company, or in one of the Liverpool traders, would be in itself beneficial, but an invalid should never go there unless he is prepared to spend more money than is required for a comfortable residence in Europe, nor without, at least, one friend prepared to fight for him and get him what he wants in a city where one is more at the mercy of hotel-keepers, and where hotel-keepers have less mercy for invalids than in any place with which we are acquainted. So protected, however, he can get fairly good food and accommodation even for an invalid, he will have plenty to amuse him; if the comparatively dry air of Cairo suits him, he can run over by rail to the top of the Red Sea, and spend a week or two at Suez,* where, provided his friend is still at his elbow, he will be well lodged and fed, and after spending November and December in Cairo or Suez, if strength has returned, and cough is not troublesome, and if he is very prudent in not exposing himself, he may secure a cabin in one of the steamers which every year make a trip up the Nile, this will occupy about twenty days, and cost him about £40, and will give him quite enough of the Nile. Then in the beginning of March, if he have money and leisure (and those who have not should hardly go to Egypt) we strongly recommend a Syrian tour; he can go from Alexandria to Jaffa, enter Syria there, and reach the coast again at Beyrout early in May. He will be able, without delay, to return to Europe by one of the numerous steamers which touch there on their way to Constantinople or some continental port. Of course when we speak of a Syrian tour, we take it for granted that the invalid has, during his residence in Egypt, completely lost all febrile symptoms, has almost, if not altogether, got rid of cough, and has regained a fair amount of strength; but for a young gentleman or lady who has so far recovered, whether the winter has been spent in Egypt or in the South of France, we know nothing more likely to bring back health and spirits than the life of a tourist in Syria, in the saddle by day and in a tent by night, appetite is never wanting, and, if a good dragoman has been secured, an excellent table can be had, the weather, moreover, at this time is generally steady, and the country looks well. For such a tour

* The air at Suez is extremely dry and does not suit some, we have seen an elderly lady with chronic bronchitis and profuse expectoration, attacked a few hours after her arrival with intense dyspnea, all secretion being arrested; she became quite comfortable again as soon as she was removed to the moist air of Alexandria. We understand the Peninsular and Oriental Company have now in their own hands the Suez Hotel, if so invalids may calculate on the best accommodation.

the party should consist of four persons at least, and, after an attentive study of "Murray," their arrangements for the journey should be carefully made. The invalid whose health on leaving Egypt is too insecure for attempting Syria, may take ship at Suez for Australia, or at Alexandria for England.

Winter and Spring on the Shores of the Mediterranean is charmingly written, and contains an account not only of Mentone, but of the health resorts of Sicily, Corsica, Algiers, and Spain, and, allowing for some pardonable enthusiasm on the part of Dr. Bennet, may be accepted as a guide, while in Dr. Williams's *Climate in the South of France* readers will find a sufficiently full and, in our opinion, a fair and impartial account of the various towns. Hyeres, Cannes, Nice, Mentone, and San Remo, along the northern shore of the Mediterranean. Those who wish to make themselves up on the subject should read what he says. Nice is, in our opinion, the least to be commended on account of its exposure to keen winds and sudden changes of temperature. In our table it will be seen that Hyeres is separated from the group, and out of this no small advantage arises. Those consumptives in whom the lull in the course of their malady is hardly complete enough to justify their removal to a climate which has so little of what is soothing in it as that of Cannes or Mentone, might on first reaching the south of France take up their quarters at Hyeres.

Those who take much interest in the subject of change of climate for the phthisical will do well to read Dr. Weber's paper in the *Medico-Chirurgical Transactions*. He adduces abundant evidence of the curative influence of residence at Jauja, in the Peruvian Andes,* at a height of ten thousand feet above the sea level, and supplies numerous observations, which go to show that St. Moritz, in the Engadine, and other elevated regions exercise a similar effect, though less powerfully. Physicians occasionally meet sufferers who could go to one of these localities without inconvenience, and would willingly try it, or who may even be able to make a residence there subservient to their business or the exercise of their profession. For a most satisfactory description of St. Moritz we refer our readers to Dr. Yeo's work. In his estimate of its advantages and disadvantages we are inclined to agree. Of its effect on those who had pulmonary complaints we have no experience, but we have known a month spent there in summer

* See also a most valuable paper in a former number of this Journal (May, 1866), by Dr. Archibald Smith.

materially help young people who, without any particular ailment, were dyspeptic and spiritless.

On other continental wintering-places we offer a few observations in the table.

And now, it may be asked, is the climate of the British Isles so unfavourable that consumptives cannot hope in them to battle successfully for life? By no means we reply. In many cases, it is true, complete change in itself affords a valuable stimulus, and for those who every year while in health have been accustomed to a change, going from town in the summer to the sea-side, and in the autumn to a country house, it is probably desirable that when attacked by phthisis they should leave these islands altogether; but others, and especially those whose usual residence is so far north that they may find in other parts of the United Kingdom a climate warm compared to their own, will do quite as well in the south of England, or at Queenstown, or even at Sutton, Ballybrack, or Dundrum (county Dublin).^a

To our mind, however, the great health resort for the phthical patient is the open sea, and by exercising a little caution in the selection of a route, it is available at all seasons. Where local disease has been temporarily arrested, but appetite refuses to come, and strength is gradually failing, nothing produces so sudden an improvement as getting out into the ocean. A sailing vessel is superior to a steamer, but now-a-days sailing vessels are so little used as passenger ships that the accommodation and food on board is seldom such as a consumptive needs. Indeed, in any case, unless he is embarking in a vessel belonging to one of the great companies, the phthical sufferer should provide himself with tins of preserved soup or essence of beef. The voyage to Australia is probably the best, but before venturing on such a long one he may go to the Cape, or through the Mediterranean to Alexandria. From the advantages of this the poor are by no means excluded. We know respectable men in humble circumstances whose lives have been saved by exchanging the position of an inside servant for that of a ship steward, and we have seen a young man who had cavities in both lungs, and would probably have lived but a few months in a

^a One of the best marked instances we have seen of arrest of phthisis occurred in a young lady who, leaving a damp and bleak northern town, wintered in this little village. She was so fortunate as to find the kind of cuisine which exactly suits a phthical patient. She took cod liver oil resolutely, and she is now stout and without any symptom of illness, though the left infra-clavicular region is markedly depressed and the breathing cavernous.

counting-house, enjoying fair health for seven years as a purser's clerk.

We have devoted much more space to this subject than we originally intended; but we have not for some years referred to it at length in the *Journal*, and its importance now-a-days, when travelling is comparatively easy, can hardly be overrated. To the consumptive the struggle for life is at all times an arduous one, but in many cases it can be maintained for years, and in not a few of those, whose pecuniary resources are such as to enable them to have every advantage, the struggle is ultimately successful. A portion of lung must in every case be lost, but it may be a very minute islet, and life is not on that account less secure than in many who are anatomically undamaged, and among the means which help in this fight for existence, a judicious change of climate is not the least important, provided it be not relied on alone, but viewed merely as a means of placing the consumptive in a more favourable position for the adoption of other therapeutic measures.

JAMES LITTLE.

The Life and Letters of Faraday. By DR. BENCE JONES, Secretary of the Royal Institution. 2 Volumes. London: Longmans, Green, and Co. 1870. Large octavo, 975 pages.

IN modern experimental science no name occupies so distinguished a place as that of Michael Faraday. Born in the lower walks of life, self-taught, he overcame every obstacle of birth, education, and fortune, and raised himself to the very highest position in the world of science. Faraday was born in 1791, near London. His father was a blacksmith. He was early apprenticed to a bookseller, a circumstance which probably had some influence on his future career, as it gave him an opportunity of reading works of science. Whilst quite a boy, he acquired a taste for the then comparatively new and wonderful science of electricity, and became the possessor of a small electrifying machine. In 1812, he attended the brilliant lectures delivered by Davy at the Royal Institution, and took notes of them, which he sent to the lecturer, expressing at the same time, in a modest and diffident manner, his anxious desire to be employed in Davy's laboratory. At first Davy discouraged the young aspirant, but finally, yielding to his ardent entreaties, he placed him as an assistant in the laboratory of the Royal Institution.

Some time after this appointment he accompanied Davy to the continent as his amanuensis; and on this occasion his quality of temper appears to have been somewhat severely tested by the petulance and exactions of Lady Davy. "Her temper," says Faraday, "oftentimes makes it go wrong with herself and with Sir Humphrey." On his return from this prolonged continental tour, Faraday was entrusted by Davy with the carrying out of some important investigations, during which he made his first great discovery—namely, the possibility of liquifying some of the so-called permanent gases. In 1832, and at the age of 41, he was appointed Fullerian Professor of Chemistry in the Royal Institution; and henceforth the fame of Faraday grew brighter and brighter, until at length its rays spread to every place where science is cultivated or loved.

As a discoverer, Faraday ranks with Davy, Berzeliars, Liebig, and Newton. In almost every branch of physics he laboured with success. A mere catalogue of his discoveries and inventions would occupy several pages of this Journal. What he did, he did thoroughly. The results of his inquiries are likely to endure, for the facts which he made known to us twenty or thirty years ago are not superseded by the facts which he or others discovered at later periods. He created—if we may use such a term—the science of magneto-electricity. He was the first to prove the conservation of statical force. His original papers in the Transactions of the Royal Society during the last thirty-five years constitute nearly the whole of the history of magnetism, electromagnetism, and diamagnetism. Amongst his purely practical labours may be specialized his method of manufacturing glass for optical purposes, his arrangements for deep-sea diving, and his construction of light-house lamps.

Faraday was an exceedingly popular lecturer. His style was clear and simple. He had a ready utterance, and he possessed the rare quality of being able to express his ideas in the fewest possible words, without at the same time exhibiting anything approaching to baldness of style. Of the moral character it would be difficult to speak in terms too favourable. He was eminently pious; and he did not allow his earthly sciences to interfere with his devotion to the study of things "which are not of this world." Truly, it may be said of this great philosopher that he approached perfection as nearly as it is possible for creatures swayed by human passions to do. He was pre-eminently truthful, generous, unselfish,

unenvious. He was endowed with a rare mental organization. His biographer says that his imagination sometimes approached divination. His manners were kindly and unaffected, and the high honours which he received exercised not the slightest corrupting influence upon him. Faraday died on the 25th August, 1867. He was married, and he had many dear relatives. All Britain mourned his death; and his departure left a great blank in the world of science.

Dr. Jones has carefully discharged the somewhat difficult duties—less difficult, however, than usual in the case of Faraday—of a biographer. He had ample opportunities of becoming thoroughly acquainted with Faraday and his thoughts and deeds, having been for many years secretary to the Royal Institution, and a most intimate friend of the illustrious philosopher, whose career he has so faithfully and so graphically written.

RECENT WORKS ON SANITARY SCIENCE.

1. *Fourth Annual Report of the Metropolitan Board of Health of the State of New York*, 1869. New York: Appleton and Co. 1870.
2. *Investigations in the Military and Anthropological Statistics of American Soldiers*. By BENJAMIN APTHORP GOULD. New York. Published for the U. S. Sanitary Commission, by Hurd and Houghton. 1869.
3. *Health of the Navy*. London. 1870.
4. *Annual Report of the Medical Officer of the Privy Council*. London. 1870.
5. *Public Health—A Popular Introduction to Sanitary Science*. By WILLIAM A. GUY, M.B., Professor of Forensic Medicine and Hygiene in King's College, London. London: H. Renshaw, Strand. 1870.
6. *A Resume of the History of Hygiene*. By W. H. CORFIELD, M.A., M.B., Professor of Hygiene in University College, London. London: H. K. Lewis, Gower-street. 1870.
7. *Lectures on Food*. By H. LETHEBY, M.B., &c., Medical Officer of Health and Chemical Officer of the City of London. London: Longmans, Green, and Co. 1870.

8. *Population: Its Law of Increase.* By N. ALLEN, M.D., Lowell, Massachusetts. 1870.
9. *Physical and Medical Topography, &c., of Wheeling (United States).* By J. E. REEVES, M.D., Wheeling. 1870.
10. *Reports on the Health of Leicester, for 1867, 1868, and 1869.* By J. W. CRANE, M.D.
11. *Nekrosozoic Process.* Reports by Dr. F. DELAFIELD and Professors J. R. WOOD, R. O. DOREMUS, and A. FLINT, jun. London: A. Garstin and Co., Welbeck-street.
12. *Various Reports on Sanitary Matters.* By Dr LOGAN Medical Officer of the California Board of Health.
13. *Opening Remarks.* By the President (Dr. RUMSEY) of the Public Medicine Section of the British Medical Association. August, 1870.
14. *A Digest of Facts relating to the Treatment and Utilization of Sewage.* By W. H. CORFIELD, M.B., &c. London: Macmillan and Co. 1870.

THE greatly increased attention given, of late years, to sanitary matters has re-acted upon the literature of that branch of medical science, and works and periodicals on public and private hygiene are now becoming very numerous throughout Europe and North America. We give here the titles of a few works, relating more or less intimately to public health, but their contents will be dealt with fully, and to a great extent explained in our reports on public health. In this number we largely make use of the valuable information given in Dr. Simon's report, and we have freely borrowed, with due acknowledgement, from some of the others.

Of Dr. Simon's annual reports, it would be indeed difficult to speak in terms too laudatory; they are a credit to the country, as well as the able authors whose writings enrich their pages. The present report is one of almost more than usual interest. The huge Blue-book on the Health of the Navy contains a great variety of useful statistical information. The report of the New York Board of Health contains 594 pages, and is certainly a most creditable addition to recent works on hygiene. The New York Board of Health has been only four years in existence. It is composed of five sanitary commissioners (all medical men), and four police commissioners. Its staff is composed of a secretary, a sanitary superintendent, two assistant sanitary superintendents, and ten sanitary inspectors (all except the

secretary, medical men), counsel, attorney, assistant attorney, engineer, chemist, chemical disinfecter, corresponding (medical) secretary, chief clerk, register and deputy register, clerk, and a large number of subordinate officials. The revenue of the Board is about £30,000 a year. In a future number we purpose describing the sanitary operations carried out by this admirably constituted public health body. The Californian Board of Health was only instituted last year under the direction of an able sanitarian—Dr. Logan.

Dr. Guy's book contains a graphic description of the various epidemics which ravaged the population of these countries, from the earliest period until the end of the eighteenth century. Dr. Corfield's pamphlet gives the admirable inductory lecture to his course on public health, delivered by the author last May, in University College. Dr. Rumsey's pamphlet contains the interesting remarks on State medicine made by him at the late meeting of the British Medical Association.

Dr. Letheby's work is an enlargement of the four extremely interesting "Castor Lectures" delivered by the author two years ago. It contains a great variety of most valuable and recent information relative to food and diet.

In Dr. Allen's pamphlet, the author shows that in parts of the United States the conditions for the rapid increase of the population ought to exist, but that from a variety of causes which he points out, and which are remediable, the population is not increasing, except by immigration.

Nekrosozoic process is simply a plan of embalming the human body, which appears to have been tried with great success in the United States. The process consists in applying, *not* injecting, a fluid, which appears to possess very great preservative and disinfecting properties. Perhaps this wash would prove useful in the case of the bodies of those who had died from contagious diseases.

RECENT WORKS ON MENTAL DISEASES.

1. *The Physiology and Pathology of Mind*. By HENRY MAUDSLEY, M.D., Physician to the West London Hospital; Lecturer on Insanity of St. Mary's Hospital Medical School, &c., &c. Second Edition, revised. London: Macmillan & Co. 1868. 8vo, pp. 516.

2. *The Pathology and Therapeutics of Mental Diseases.* By J. L. C. SCHROEDER VAN DER KOLK, Professor of Physiology in the University of Utrecht. Translated from the German by JAMES T. RUDALL, F.R.C.S.E., &c., &c.
3. *On the Obscure Diseases of the Brain and Disorders of the Mind.* By FORBES WINSLOW, M.D., D.C.L., Oxon. (Hon.), &c., &c. Fourth Edition, revised. London: John Churchill & Sons. 1868.

THE treatise of Dr. Maudsley has already reached a second edition, and has been translated into German, by Dr. Böhm. The remarkable merit of the book has thus been already recognized, both in these countries and abroad. Written with great clearness and elegance, and evidencing a thorough mastery of the intricate subject of which it treats, it must be regarded as one of the most important contributions to the literature of insanity, which have appeared in recent times.

The first part of the work is devoted to an inquiry into mental phenomena from the physiological point of view, and although we differ with the author as to the low estimate which he has formed of the value of the psychological method of investigation, we regard it as very important that the physiological method should be made to yield as abundant fruit as possible, and that the subject should be thoroughly worked out on this basis. This portion of the treatise will be found to contain many striking and ingenious views, as well as a tolerably complete account of the principal inductions which have been arrived at. It is this section which will be read with most interest by the profession at large; the second portion of the work, treating of insanity, being likely to interest a more limited class.

The chapter devoted to the spinal cord, will be found to contain the essence of Dr. Maudsley's doctrines regarding the education of the nervous centres by experience. The following observations regarding the cord, which will give a notion of the author's manner of dealing with his subject, seem to us to be eminently just and cogent:—

“In the registration of impressions made upon it, in the assimilation of their residua, there is slowly embodied a quantity of energy as an organic addition of power; force is being stored up in the gradual organization of its faculties. The exhaustion which we feel from our efforts to acquire any particular skill of movements, as in learning to

dance, the labour given to the frequent voluntary repetition of the stimulus and adapted reaction thereto, until by practice the definite relation has been established, and the desired skill acquired;—these testify to the expenditure of so much force which has been laid up as statical power in the constitution of the ganglionic cells of the cord, rendering possible for the future a group of associated movements in answer to a moderate and, as might often seem, disproportionate stimulus from without. Like the brain, the spinal cord lays up a good store of power in its memory. Man's life truly represents a progressive development of the nervous system, none the less so because it takes place out of the womb instead of in it. The regular transmutation of motions which are at first voluntary into secondary automatic motions, as Hartley called them, is due to a gradually effected organization in the proper centres; and we may rest assured of this, that co-ordinate activity always testifies to stored-up power either innate or acquired."

From Dr. Rudall, surgeon to the Melbourne Hospital, we have a translation of a short treatise on mental diseases by the late Professor Schroeder Van der Kolk. This was the last of the numerous labours of its celebrated author, and was left unfinished at his death. The work has been rendered complete by re-printing two previously published papers, in which the author had treated those portions of the subject which were found to be wanting in the manuscript.

The greater portion of the treatise is devoted to the physiology and pathology of the brain, and will be found to contain a great deal of interesting and valuable information on these subjects detailed in a clear and methodical way, together with a number of original observations by the author himself.

The remarks on the pathological changes in the brain, are of especial importance, as Schroeder Van der Kolk had devoted extraordinary care to the investigation of the minute anatomy of the nervous centres, and was deservedly considered as one of the very highest authorities on this subject. He adopts a division of the cerebrum according to function, which, in so far coincides with that of Gall, that the anterior lobes are regarded as being connected with the intellectual faculties.

The author states that in cases of intellectual insanity, he has invariably found the grey cortical substance of the anterior lobes presented morbid changes, being either more deeply coloured, more adherent to the pia mater, or softened. In melancholia, on the other hand, he has found that the cortical substance of the upper and posterior portions of the hemispheres exhibited traces of disease

If future observation should confirm this statement, it will be an extremely important advance in our knowledge of cerebral pathology; but there are so many conflicting observations, that we must, for the present decline to receive this as the expression of a general law.

A very interesting point in the author's remarks is with regard to idiopathic inflammation of the dura mater. This disease, which is usually hardly noticed by writers, is, according to the author, much more frequent than is generally supposed. He details fully the history of eight cases, in which he believes this affection to have existed, and summarizes some of the more important points regarding the malady, as follows:—

“From my experience, which is not entirely exhausted by the preceding cases, I cannot hold idiopathic pachymeningitis, independent of external injury or syphilis, to be so rare a disease as authors affirm. I believe the disease is frequently mistaken, and supposed to be a *febris larvata*, on account of the regular intermissions, or more frequently a *cephalæa rheumatica*.

“At first sight it may appear strange that this inflammation is distinguished by such intense painfulness. It must be remembered, however, that the *dura mater cerebri* consists of two layers, of which the outer forms the periosteum with which the *dura mater proper* is coherent. The great painfulness in consequence of inflammation, is possessed by the *dura mater* in common with the periosteum of other bones. The *dura mater* of the vertebral canal, separated from the periosteum, is, according to my experience, far less painful in inflammations, than the *dura mater cerebri*. Also degenerations, ossifications, and even inflammations of the *falx cerebri* appeared in a few cases which have occurred to me, not to pursue a very painful course. In the vertebral canal, an isolated inflammation of the *dura mater* occurs indeed only seldom, and on that account we have no perfectly pure observations. However, I have not observed the pains occurring here in such severity, although, perhaps, they proceeded from other parts. If the disease takes a more chronic course, through which the *dura mater* unites almost inseparably with the skull, then the severe pains do not always occur. * * *

“The intermittence is also peculiar, it often occurs as distinctly periodic as in intermittent fever, but mostly manifests itself irregularly, so that rather long complete intermissions are distinguished. Here, again, we recognize the correspondence of the *dura mater*, with the periosteum of other parts. In periostitis generally, the pain comes on more severely during the night, or it has even longer intermissions. Other authors also mention the intermittence of the symptoms of the

disease. Especially many observations of the kind are found in the works of the distinguished Lallemand."

The following remarks on the classification of insanity, will show the division adopted by the author:—

"We are accustomed to compare the different kinds of intellectual confusion according to the differences of the phenomena which they call forth, and to note them down as mania, monomania, melancholia, dementia, and idiotism. This classification certainly serves to distinguish the different forms, and deserves to be retained; however, it has not always appeared to me to be quite practical, because it proceeds more from the morbid symptoms than from the nature and origin of the disease. For some years, I have therefore reduced the different forms of the disease to two principal groups, which may be designated as idiopathic and sympathetic insanity, which are distinguished from one another by special characteristics, and which serve all considerations in a therapeutical point of view.

"In idiopathic insanity the brain suffers primarily; it may have for its origin, unusual mental exertion and over-excitement of the brain, or may have been occasioned by some violent influence, such as a fall, a shock, or by a certain tendency, and not unfrequently by an hereditary predisposition.

"Sympathetic insanity exists, when the brain suffers only secondarily, and the exciting cause lies in other parts of the body, especially in the abdomen, or in the sexual apparatus. By long continuance, idiopathic insanity may proceed therefrom; recovery may not occur, unless the remote causes have been got rid of. Hence results the great practical utility of this classification."

The translation bears some evident marks of haste, for which the professional duties of Dr. Rudall perhaps offer some excuse. In several instances, the desire to be literal has led the author to employ modes of expression, which are scarcely intelligible. It is not very easy, for example, to catch the meaning of such a sentence as the following, which is found in page 36.

"Suppose a good friend, in rapid course, panting and heated, were to reach us."

Of Dr. Forbes Winslow's well known treatise, it is sufficient to say, that this, the fourth edition, contains some additional matter, especially with regard to the pathology and treatment of epilepsy, progressive locomotor ataxy, aphasia, and glosso-laryngeal paralysis.

PART III.

QUARTERLY REPORTS.

REPORT ON MEDICINE.*

By JAMES CUMING, M.A., M.D.; Professor of Theory and Practice of Medicine, Queen's College, Belfast; Physician to the Belfast General Hospital; Examiner in Medicine in the Queen's University in Ireland.

LOCAL TREATMENT OF DILATATION OF THE STOMACH.

A METHOD of treating a painful and intractable form of gastric disease, which promises to be of great practical value, has been proposed by Professor Kussmaul,^b of Freiburg, and has been extensively employed in Germany, and with very considerable success. In cases in which, owing to constriction at the pyloric orifice, an obstacle is presented to the exit of the contents of the stomach, the organ becomes dilated sometimes to an enormous extent. The retention of a quantity of the contents of the stomach allows changes of a fermentative character to occur; *sarcinæ*, as is known, are frequently found to be present; nutrition becomes seriously interfered with, and great distress is experienced by the patient. It occurred to Kussmaul, when treating a case of this kind, that if he could completely empty the stomach of the irritating and fermenting material which it contained, much suffering might be saved to the patient, and that the chances of a healthy condition of the gastric mucous membrane being brought about would be materially increased.

* The author of this Report, anxious that every contribution to Pathology and Practical Medicine should be noticed, will be glad to receive any publications on these subjects. If sent to correspondents of the Journal they will be forwarded.

^b Dr. Kussmaul's first communication on this subject was made in 1867, at the *Versammlung Deutscher Naturforscher und Aerzte*. A full account of his views, with several highly interesting cases, will be found in the *Deutsches Archiv für Klinische Medicin*, Dec., 1869. Cf. also, Jürgensen, *zur Localtherapie der Magen-Krankheiten*. *Ibid.*, April, 1870. Wiesner, *die Behandlung der Ectasie des Magens*. *Berlin Klin. Woch.*, Jan. 3, 1870.

The first occasion on which he had an opportunity of employing this plan was in the case of a young woman, aged twenty-five, who had long suffered from dilatation of the stomach, probably as a consequence of ulcer at its pyloric end. There was chronic catarrh of the gastric mucous membrane, and on palpation a small, smooth, movable tumour was recognizable in the situation of the pylorus. The patient vomited daily three or four pints of an acid, grayish-brown matter, in which *sarcinæ* could be detected. There was a feeling of burning in the stomach, the bowels were confined, the patient was extremely weak and exhausted. She had become much emaciated, and had exhibited some remarkable spasmodic symptoms. A carefully regulated diet and the usual medicinal agents had been tried perseveringly, but had altogether failed to give more than temporary relief. Under these distressing circumstances Kussmaul thought that if the irritating contents of the stomach were removed by mechanical means, it was likely that the bulk of the distended organ would diminish, and that thus a chance might be afforded to the patient. This ingenious idea was put in practice, the tube of the stomach-pump was introduced, and the contents of the stomach completely evacuated, nearly six pints of a sour, dirty, gray fluid, containing *sarcinæ*, with softened and decomposed portions of food, being withdrawn.

After the stomach was emptied Vichy water was thrown in, and again removed by the pump, so that the organ was thoroughly washed out. For the two days which followed the pumping the relief was complete, and the return of the symptoms which occurred was again relieved by a similar procedure at intervals of two or three days. In a fortnight her condition had improved so remarkably that she might be described as a different person. In two months she had gained 15 lbs. in weight, and ultimately recovered completely. She gained flesh and strength, and the little tumour which had been perceptible in the pyloric region could no longer be detected. Two years have since elapsed during which no gastric symptoms whatever have supervened.

Several other cases are recorded by Kussmaul and others, in which the employment of this method of treatment has been followed by permanent cure of dilatation of the stomach.

Several interesting facts have been noted during the observation of these cases. It was found that even after profuse vomiting the dilated stomach retained a considerable amount of contents. In one instance, after two pints of acid fluid had been thrown off,

the introduction of the stomach-pump was followed by the withdrawal of between five and six pints of a similar liquid. A second point of some interest is that occasionally sarcinæ were found in the fluid removed artificially, although none could be detected in that expelled by vomiting. It is remarkable also how persistent the existence of sarcinæ was found to be. In one case these organisms were found to continue, notwithstanding a daily washing-out of the stomach with solution of soda, hypo-sulphite of soda, or creosote water. It was also found that a distinctly recognizable tumour, presumably at the pylorus, and which had given rise to the suspicion of malignancy, completely disappeared in more than one instance after the gastric symptoms had been relieved.

The process of dilatation of the stomach, from narrowing of the pylorus, is explained by Kussmaul in the following manner. Even when the constriction is only slight the force of the stomach becomes exhausted before it has completely expelled its contents, so that a small residuum remains; this becomes greater and greater, so that ultimately the stomach, in addition to the resistance at the pyloric orifice, has thrown upon it the burden of a large quantity of contents, which may reach, or even exceed, 10 or 11 lbs. in weight. Portions of this large quantity are, no doubt, got rid of from time to time by vomiting, but only to again accumulate from the ingesta. The muscular elements of the coats of the stomach become in many instances hypertrophied, and in this way the dilatation may be postponed for a longer or shorter period. Notwithstanding the hypertrophy, however, a period arrives at which the stomach becomes unable to propel any, or only an insignificant portion, of its contents through the pylorus. It has been observed, however, that even in cases of great dilatation of the stomach distinct movements of the organ can be perceived, which show that its contractile power, although weakened, is not altogether destroyed.

It is evident that the method proposed by Kussmaul of dealing with these distressing cases is one which offers remarkable advantages if certain practical difficulties can be got over. The introduction of the tube of a stomach-pump is a painful and repulsive process, and one to which many patients would scarcely willingly submit. But it has been noticed that in the cases in which its employment is necessary it is borne much better than would be at first considered possible. The repeated acts of vomiting, often artificially induced by patients by irritating the throat, bring about a condition of the œsophagus and pharynx which enables the

introduction of the tube to be borne with comparative ease. Kussmaul relates a case of much interest in which an individual learned to perform the operation for himself, and when any evidence of indigestion occurred was accustomed to at once empty the stomach by the pump. He observes that this refinement of modern skill casts in the shade the "*consuetudo vomitandi*" of Vitellius.

The relief experienced by the patient seems to be immediate and decided. The pain and burning sensation in the stomach altogether cease after the irritating contents of the organ have been removed, and the membrane washed with a weak alkaline solution. Nutrition becomes greatly improved. So long as the stomach is only partially unloaded by vomiting, all food ingested becomes at once mixed with the sour, ill-smelling residue left in its cavity. Fermentative changes of necessity result, and it is impossible that healthy, nutritious chyme can be formed. This state of matters, of course, ceases when the contents of the stomach have been removed, and its digestive power gets free play.

In cases of malignant disease, or when great cicatricial contraction of the pylorus exists, or when the stomach has undergone much degeneration, the method of Kussmaul can be only exceptionally applied, and at best can be productive merely of mitigation of the symptoms. The cases in which it proves of most signal service are those in which only a moderate amount of constriction exists, depending probably in the majority of cases on chronic gastric ulcer. It is difficult, however, to estimate the degree of narrowing of the pylorus during life. Even when the symptoms were marked, and when long-continued constipation had existed, leading to the belief that very little could have possibly passed through the pylorus, an opening capable of admitting the little finger has been found at the autopsy. This fact affords an encouragement to the adoption of remedial measures, even in apparently desperate cases.

The instrument employed by Kussmaul has been described as Fischer's pump, but is copied from that introduced in Boston by Wyman, and brought into notice in this country by Bowditch. It seems to be identical in principle with the stomach-pump, manufactured by Weiss^a as early as 1824. It has been shown by Ploss^b

^a Ziemssen die Weiss'sche Magenpumpe betreffend. Deutsches Archiv, April, 1870.

^b Deutsche Klinik, No. 8, 1870.

that by a simple syphon arrangement the stomach can be easily emptied, and a very ingenious and easily applied instrument has been devised by Rosenthal for the purpose of applying the principle of the syphon, in a manageable way, to the performance of the operation of emptying and injecting fluid into the stomach.

TREATMENT OF PLEURITIC EFFUSION.

The operative treatment of effusion in the pleural cavity has recently been the subject of some important observations. The value of thoracentesis—an operation which dates from the earliest times of medicine—has been very variously estimated by many of the most eminent observers. Laënnec seems to have entertained little confidence in its utility. Dupuytren performed the operation frequently, and with fatal results in the great majority of his cases, so that in his last illness, when it was proposed to him to allow his chest to be tapped for the relief of a copious pleuritic effusion which existed in it, he is reported to have refused, saying that he preferred to die by the hand of God than by the hand of man. In this country the operation was regarded with little favour, notwithstanding the energetic advocacy of a more extended application of it by Dr. Thomas Davies and Dr. Hamilton Roe. It is to Trousseau probably more than to any other that the more frequent employment of this procedure in recent years is owing. Dr. Bowditch, of Boston, has also performed thoracentesis very frequently, and with great success, and his experience has had much influence in popularizing the operation. Nevertheless, there can be no doubt that there is a wide divergence between the practice of physicians in this kingdom and that adopted on the Continent with regard to tapping the chest.

The opinion prevalent in this country seems to be that thoracentesis is not to be had recourse to during the acute stage of pleurisy, merely in consequence of the effused fluid having reached a large amount, unless other urgent symptoms, such as marked dyspnea, have arisen. On the other hand, Trousseau has laid it down as a rule, that the chest must be at once tapped in all cases in which there is dulness from the base of the lung to the clavicle in front and to the supra-spinous fossa behind, with displacement of the diaphragm, liver, spleen, or heart. The danger which he apprehends from this condition and which he regards as

* *Berliner Klinische Wochenschrift*, June 13, 1870.

rendering the operation imperative, is that of sudden death apparently from syncope. Some doubt^a has been thrown on the likelihood of this mode of fatal issue being a legitimate result of the disease, and it has been suggested that antiphlogistic measures adopted to combat the malady may have had much to do with the production of syncope. The termination of acute pleurisy in sudden death has however been too often observed in the absence of any lowering treatment to render this explanation satisfactory. Quite recently Dr. Sutton^b has recorded a case in which death suddenly occurred in acute pleuritis with only a moderate amount of effusion, not exceeding forty ounces. Another case of a similar kind occurred within the cognizance of the writer of this report. Trousseau has suggested that the explanation may be that the displacement of the heart by the pressure of the fluid may cause such a degree of tension of the large vessels that the passage of the blood through the aorta may be materially interfered with, and that as a result of any unusual effort on the part of the patient the circulation may be altogether arrested; or that in some instances it may occur in consequence of the diminished flow of blood through the vessels giving rise to the formation of thrombi in the cavities of the heart.

Bartels,^c who has contributed a paper of great value on the question of the operative treatment of pleuritic effusions, has had several opportunities of making *post mortem* examinations of the changes produced by pressure in the position of the heart and great vessels. He states that the most important effect as regards the circulation is that produced on the intra-thoracic portion of the inferior cava and on the right auricle of the heart, and draws attention to the fact that when effusion takes place into the left pleura it has more influence in producing stoppage of the circulation than when it occurs in the opposite side. In a case of left-sided pleuritic effusion Bartels found that the heart had been pushed to the right side, so that it assumed an almost vertical position with the apex resting on the depressed diaphragm, that the lower wall of the right auricle was folded on itself, and that the inferior cava immediately above its emergence from the foramen quadratum was bent at right angles. This condition of parts must have interfered considerably with the return of blood to the heart, and the effect of

^a Gairdner, Clinical Medicine, p. 374.

^b British Medical Journal, July 17th, 1870.

^c Deutsches Archiv für Klinische Medicin. Band. iv.

any sudden exertion under such circumstances might readily be to altogether cut off the flow of blood through the cava for a short time, and thus bring about fatal syncope.

In many cases of acute pleuritic effusion, it has been found that after tapping the fluid does not again accumulate. Trousseau has recorded instances of this, and the same fact has been noted by other observers. An interesting case^a in which tapping the chest was followed by complete and rapid recovery of the patient, has been quite recently recorded by Dr. Constantine Paul. In this case the enormous amount of above nine pints of fluid were withdrawn from the chest at a single operation. No fresh accumulation took place, and the patient recovered without a single symptom of disturbance from the thoracentesis.

Professor Kussmaul^b reports with great care and fulness the details of sixteen cases in which thoracentesis was performed in his own practice. In six of these, in which the operation was performed under urgent and almost desperate circumstances, permanent recovery followed. Of these six cases five were owing to acute and chronic empyema, and one to acute pyopneumothorax. In a seventh case life was prolonged for a year and a half. In an eighth case, one of acute pleuritis with purulent effusion, the first operation decidedly injured the condition of the patient, probably in consequence of having been too early performed; subsequent operations, however, became necessary, and the patient died phthisical a year after. The ninth case terminated favourably, but not on account of the tapping. The tenth case was an empyema of necessity, with a great amount of pericardial effusion. The eleventh, a similar empyema, with advanced disorganization of the pleura; both these cases ended fatally. The twelfth and thirteenth cases were cases of pyopneumothorax, in which the operation was only performed with the view of palliating the sufferings of the patients, which object was satisfactorily accomplished. The fourteenth case was one of tubercular pleuritis, with hemorrhage at the pleural sac, and was also relieved. The fifteenth and sixteenth were cases of acute pleuritis, with extensive serous exudations, and both ended fatally.

It becomes accordingly a matter deserving the serious attention of physicians whether the employment of thoracentesis might not be advantageously somewhat extended in cases of acute pleurisy

^a Dr. C. Paul. Bulletin Général de Thérapeutique, Dec. 15, 1869.

^b Deutsches Archiv, Band. iv.

with very copious effusion, even in the absence of symptoms of an urgent character.

With regard to chronic effusion, the arguments in favour of the operation are still stronger. In this country it is not considered advisable to tap the chest in consequence of the presence of fluid, even in considerable quantity and remaining for a long period, if there is reason to believe that it is of a serous and not of a purulent character.

There are several considerations possessing weight which are favourable to the adoption of operative interference at a comparatively early period. Certain changes of some prognostic importance have been found to take place in the ribs and their cartilages in cases of chronic pleuritis. A process of ossification of the cartilages is found^a to take place even in young subjects, and an enlargement frequently to a considerable extent of the ribs themselves. The effect of these changes may, as has been shown by Bartels, be very prejudicial to a complete removal of a chronic effusion. If the lung is bound down by false membranes so as to be unable to expand, or if the pulmonary tissue itself has undergone such changes as to render it no longer capable of expansion, then the only way in which the pleural surfaces can be approximated so as to obliterate the space between them and permit of the absorption of the fluid is by the yielding of the chest-walls. It will be easily understood that the possibility of this yielding depends in a great degree on the elasticity of the parietes, and that any change which increases their rigidity offers an obstacle which may be an insuperable one to this change of shape, without which removal of the fluid is impossible.

Other considerations favourable to an early performance of the operation are derived from the changes which a lung which has been subjected to long-continued pressure is liable to undergo. It is remarkable how completely this organ may retain its power of being inflated even after having been compressed for a long time, but it does occasionally happen that changes occur in its texture which render it no longer capable of admitting air. The false membranes, also, which bind down the lung may undergo a process of development which may render their yielding impossible, and in this way the expansion of the lung may be prevented.

It has long been known that a tubercular condition of the lung

^a Parise, *Archives Générales*. 1849. Wintrich, *Die Krankheiten der Pleura*. Virchow's *Handbuch der Spec.*, Path. 1855.

is one of the causes of pleurisy; there is every reason to believe that the converse of this proposition is also true, and that the existence of a chronic pleurisy may be a cause of phthisis. Nor is this difficult to understand by the aid of the additional breadth which our conceptions regarding the nature of pulmonary consumption have recently received. When one lung is compressed, so as to become useless, the needs of the circulation must throw such an additional amount of work on the unaffected lung as to cause a hyperemic condition of it. This tends to the production of a catarrhal condition under slight exciting causes, and it must be remembered that the free play of the unaffected lung is seriously interfered with, partly owing to pressure through the mediastinum, partly to the pain caused by the respiratory movements, partly to the interference with the diaphragm by the fluid effused. These are precisely the conditions which favour an accumulation of the products of bronchial catarrh within the lung, and the development of those morbid processes which are now regarded as constituting one of the most frequent forms of pulmonary consumption.

It is a fact well known to practical physicians that effusions, which there is every reason to believe to be merely serous, will for a long time resist all measures adopted for bringing about their absorption. In many instances they cause no apparent injury to the health or strength of the patient; in others they merely diminish his capacity for exertion, and render him unable to rapidly ascend stairs and the like, but otherwise seem to exercise little prejudicial effect. In such cases the question arises how far a physician is justified in leaving matters in *statu quo*, after rest and tonics and diuretics and iodine have failed in causing absorption. It cannot be a matter of indifference to the future of a patient to have a dislocated heart or a depressed liver, even if he does not suffer from the abnormal condition of these important organs at the time. Besides there is always a risk that under the influence of intercurrent disease the fluid in the chest may become purulent. No physician would hesitate to remove such an effusion if it could be done without exposing the patient to serious risk. In a case recently under the care of the writer effusion was found to exist in the left pleura, displacing the heart to the right side and causing dulness as high as the clavicle. It had accumulated without any marked chest symptoms, and also without any considerable febrile reaction. When the patient came under observation he had been blistered over

the affected side and otherwise treated with a good deal of activity. Iodide of iron, diuretics, rest, and abundant nourishment were prescribed with no perceptible effect upon the amount of fluid or on the measurement of the chest. The question arose how long it was justifiable to proceed with remedies of this kind under the circumstances. The patient was to all intents and purposes well, but unable to work from the dyspnea which exertion caused, and he had a family dependent on his exertions. Thoracentesis was decided upon, a fine tubular needle was introduced between the 6th and 7th ribs, and 20 ounces of serum removed by suction, by means of an instrument to be described. Two days after a similar amount was removed. The little operations were almost painless, no suffering being caused except by the trifling prick of the needle. No cough or other unpleasant concomitant occurred, and absorption proceeded rapidly, so that in a week the patient left hospital, and in another fortnight very little dulness and no displacement of the heart remained.

Professor Ziemssen,^a of Erlangen, recommends strongly that thoracentesis be performed even in cases of non-inflammatory hydro-thorax occurring in connexion with cardiac or pulmonary disease, with tumours in the chest, or with Bright's disease, when the pressure on the lungs has attained such an extent as to give rise to deficient aeration of the blood or insufficient circulation in the lung. In accumulations of fluid under these circumstances the operation can be considered as merely palliative, but it has been found to afford a considerable amount of relief. Ziemssen records the case of a patient in whom there were present mitral insufficiency, hypertrophy of the heart, granular kidneys, and general anasarca, and in whom hydro-thorax was present in both sides. The chest was punctured during his illness sixteen times with marked relief to the dyspnea. In these different operations above 20 pints of fluid were removed, and Ziemssen expresses his decided belief that life was considerably prolonged as well as rendered much more comfortable by his interference.

The practical rules laid down by Bartels regarding the selection of cases in which thoracentesis is to be performed, are worthy of consideration.

In all cases of simple serous effusion, accompanied by signs of displacement, the operation is requisite if the physical signs show that absorption has not commenced within a moderate time.

^a Ziemssen, *Deutsches Archiv*, Vol. v., 457.

It is not advisable to operate as long as febrile symptoms are present, unless there be urgent symptoms, such as distinct and considerable embarrassment of the circulation or of the respiration.

The entrance of air into the pleural cavity is to be carefully prevented in cases of serous effusion.

Purulent effusions are best treated by the establishment of a large fistulous opening, which permits a continuous discharge of the thoracic contents. If these effusions are removed by the trocar they rapidly accumulate afresh and exhaust the patient.

If on puncturing the chest an effusion which had been regarded as serous is found to be purulent, it is advisable to remove the trocar and make a pretty large opening at once.

The effusion is almost invariably purulent if pleurisy has occurred in connexion with pyemia, puerperal fever, and the like, if a febrile condition continues without any other cause after the effusion has ceased to increase, and is certainly purulent if œdema of the subcutaneous cellular tissue exists on the affected side.

If pneumo-thorax co-exist with purulent effusion, the operation is indispensable to prevent the contamination of the system by septic fluids.

To prevent septic infection it is necessary to cleanse the pleural sac daily, either by injections of water or of a weak solution of common salt, or by insufflation of air.

As regards the choice of an instrument, and the mode in which the operation is to be performed, we may be permitted to say a few words, inasmuch as this may be regarded as common ground for the physician and the surgeon. Opening the cavity of the thorax by means of a bistoury is reserved for those cases in which a permanent fistulous opening is required. The trocar is the instrument usually employed, and Wyman's instrument has been frequently used in connexion with the trocar.

The present writer regards an instrument which has very recently come into use as affording facilities for the safe performance of the operation, such as did not before exist. It consists^a in a

^a A good deal of discussion has taken place regarding priority of invention with respect to this instrument. The instrument used by the writer is that called the Pneumatic Aspirator of Dr. Georges Dieulafoy, of Paris, and has been patented by the Messrs. Weiss, the well-known instrument makers. An instrument identical in principle, and closely resembling it even in detail, was exhibited by Dr. Protheroe Smith at the recent meeting of the British Medical Association in Newcastle-upon-Tyne. Dr. Smith claims to have invented and employed this instrument several years before Dr. Dieulafoy's introduction of his aspirator. A commission of the

strong glass tube, about the size of an ordinary stomach pump syringe, and fitted up in the same manner with two cocks. In this is an air-tight piston, by pulling up which, with the cocks closed, a vacuum can be formed. For the instrument is arranged a series of tubular needles and trocars, beginning at the ordinary size and decreasing to the finest calibre compatible with sufficient strength. In examining for a pleuritic effusion, it is necessary to introduce the needle into an intercostal space sufficiently far to cover the little lateral openings in it. Then it must be connected with the air-pump, and the cock communicating with the interior of the pump opened, so that the cavity of the needle forms a part of the vacuum. The needle is then to be slowly pushed forward. In this way the tissues are traversed with a tube connected with a vacuum, and the instant fluid is met with it immediately rushes into the glass pump, and its character can be at once recognized.

The advantages of this instrument are at once apparent. Among the objections to the operation is certainly to be reckoned the possibility of an error of diagnosis. Now, although in the vast majority of cases error is by no means likely, it must be remembered that it is far from impossible. The case of a distinguished member of the profession, Dr. Dolbeau, of Paris, has recently attracted a good deal of attention, and has been freely commented upon by the Parisian medical press. In the case^a of this gentleman fluid was believed by some of his medical attendants to be present in the pleura, and although there was a difference of opinion as to the diagnosis, it was determined to puncture the chest. Two punctures were made without result, and the needle penetrated the lung in both instances without producing any bad effects. Twelve days later a considerable amount of fluid was found and removed by operation. It was the opinion of Barth, an auscultator of the highest eminence, that fluid was present in the first instance, but that local adhesions had occurred at the sites of puncture. It is known that even in the hands of Laënnec the liver was once perforated by a trocar in an operation

Academy of Medicine of Paris, consisting of Messrs. Broca, Jules Guérin, and Denonvilliers, examined into the question of priority, and reported at the meeting of July 27 of the present year, that an instrument identical in principle had been exhibited by M. van den Corput, Professor in the University of Brussels, as early as 1855, and that M. Laugier, the well-known surgeon, had also about the same period employed a similar instrument. The report of the commission will be found in the *Archives Générales* for September.

^a *Gazette Hebdomadaire*, May 6, 1870.

of this kind. An advantage of no mean importance in the employment of this instrument consists in the harmlessness of such errors; as the puncture of the lung by the fine needle employed is not found to be followed by any evil results.

Another advantage is the comparatively painless character of the operation. Not only is the puncture itself attended with very slight pain if a fine needle be used, but the removal of the fluid, which takes place very slowly and gradually, is not accompanied by the troublesome paroxysms of cough which have been noticed to attend the evacuation of the chest by the ordinary means. These points were brought under the notice of the profession by Blachez,^a who advocated an operation which he described as capillary thoracentesis. The admission of air is also completely prevented, and although the experience of those who have employed the operation repeatedly does not point to this accident as one of much importance, still it must be regarded as a thing to be, if possible, prevented.

Experience has shown that it is not by any means necessary to withdraw the whole of the effused fluid. The removal of a portion is in many cases all that is necessary for the prevention of absorption. Once the excessive pressure has been diminished the increased activity of the absorbents becomes sufficient for the task to which they had been before unequal.

A method of employing the syphon-principle in the removal of fluid from the pleural cavity in combination with a mercurial pressure-gauge, which enables the operator to ascertain at any moment the degree of fluid pressure within the pleura, and the amount of syphon-power employed has been suggested by Dr. Douglas Powell.^b If the syphon-principle be adopted in the operation of thoracentesis, the pressure-gauge will certainly be found an important aid in regulating its application; but there is no very obvious advantage to be gained by the employment of the syphon. An exhausting syringe seems to afford an equally efficacious, as well as a more convenient and manageable mode of withdrawing the fluid.

An instance of a totally different method of dealing with pleuritic effusion has been reported by Glauert,^c from the Clinic of Niemeyer. The treatment adopted was a modification of what is known in Germany as the Schroth cure. The patient was forbidden

^a Bulletin gén de Thérapeutique, Nov. 15, 1868.

^b Transactions of the Clinical Society of London. Vol. iii., p. 240.

^c Berlin Klinische Wochenschrift, Feb. 7, 1870.

to take any drink or fluid nourishment whatever, and his diet consisted of bread and slightly salted sausages. The attack of pleuritis had lasted fifteen days before this treatment was commenced, and the physical examination revealed effusion in the right pleural cavity, extending upwards to the third intercostal space; all febrile reaction had ceased. During the first two days of the treatment the patient suffered greatly from thirst, and was scarcely able to swallow anything, owing to the dryness of his mouth. He adhered, however, rigorously during three days to the directions, given him, with the exception of licking with his tongue some of the moisture deposited on the inside of the window panes.

During the three succeeding days he was allowed half a pint of wine daily. The amount of urine secreted during each of these three days averaged only fourteen ounces and a half. Absorption of the fluid took place rapidly. On the fourth day of the treatment friction sounds re-appeared, and on the sixth day dulness was only recognizable immediately above the liver. The patient was now permitted to eat and drink as he pleased, and in two days was completely well. His appetite and general condition were good, and all traces of effusion had disappeared. It seems from the experience of German physicians that this highly irksome and disagreeable plan of treatment can be put in practice, without risk to the future health of the patient, in the great majority of cases at least.

SCLEROSIS OF THE NERVOUS CENTRES.^a

Within the last few years much has been done to remove the obscurity in which the chronic diseases of the brain and spinal cord were enveloped, by the discrimination of several distinct varieties and forms of disease which had been previously confounded with

^a Charcot. *Histologie de la Sclérose en plaques.* Paris, 1869.

Bourneville et Guerard. *De la Sclérose en plaques disséminées.* Paris, 1869.

Bourneville. *Nouvelle Etude sur quelques points de la Sclérose en plaques disséminées.* Paris, 1869.

Archives de Physiologie Normale et Pathologique. July, 1870.

Leo. *Beitrag zur Erkennung der Sclerose des Gehirns und Rückenmarks.* Deutsches Archiv. May, 1868.

Schüle. *Beitrag zur Multiplen Sclerose des Gehirns und Rückenmarks.* Ibid. June, 1870.

Bärwinkel. *Zur Lehre von der Herdweisen Sclerose der Nerven Centren.* Archiv der Heilkunde. Virchow's Jahresbericht, Bd. 2, 1870.

Clymer. *Notes on the Physiology and Pathology of the Nervous System, with reference to Clinical Medicine.* New York Medical Journal, May, 1870.

each other. Thus progressive muscular atrophy, glosso-laryngeal paralysis, and locomotor ataxy, have been described with sufficient clearness, and defined with sufficient precision, to make their recognition a matter of comparative ease. It must be owned that no corresponding advance has been made in our methods of dealing with these formidable maladies, but there can be no doubt that progress in this direction is likely to be facilitated by the clearer and more comprehensive views which we have gained regarding the character and progress of these diseases.

To the category of chronic affections of the nervous centres, capable of recognition during life, and unhappily also to that of diseases which have hitherto resisted all therapeutic measures for their relief, we must now, it would seem, add another which presents certain characteristic symptoms, and which has also definite anatomical characters, capable of easy verification by *post mortem* examination. This disease has been variously designated as "multiple sclerosis of the nervous centres," "sclerosis insularis," "sclérose en plaques disséminées," and also by other names. On the whole, we prefer the first of these names as the most consonant with English nomenclature, and as giving, at the same time, a correct description of the morbid change. Sclerosis, we may observe, is exactly equivalent to induration or hardening.

The special anatomical characteristic from which the disease has received its name consists in the presence, in either the brain or spinal cord, or in both, of a number of indurated patches of various sizes, scattered irregularly, and apparently involving indifferently any of the different portions of the nervous centres. There are other varieties of sclerosis of the nervous centres, one being that in which the posterior columns of the cord alone are affected with induration. This is the *sclérose rubanée* or *sclerosis funicularis*, and is found to be the chief anatomical lesion in progressive locomotor ataxy. Another is the superficial form, and there are patches of induration of a secondary character met with in connexion with various morbid changes in the brain and spinal cord. It is necessary, accordingly, to employ such a designation as will give an accurate notion of the special characters of the affection which we propose to consider.

The first cases of multiple sclerosis which have been recorded are found in *Cruveilhier's Anatomie Pathologique*. Since that time numerous cases and observations have been published, principally by German and French observers. A complete bibliography of

the subject will be found in the thesis of Ordenstein, and in the work of Bourneville and Guerard. A remarkable case of the disease occurring in the person of Dr. Casper W. Pennock was reported by Dr. J. C. Morris, and the microscopical examination made by Dr. Weir Mitchell. This was communicated to the College of Physicians of Philadelphia.

The most important observations on this disease since Cruveilhier are those by Valentiner, then assistant to Professor Frerichs; by Frommann, who has published a valuable work on the subject; and above all, by Dr. Charcot, of Paris, to whom we are indebted for the elucidation of its clinical history, and for the establishing the connexion between the symptoms observed during life and the pathological appearances observed after death. An admirable account of the subject, based on the clinical teaching of this distinguished physician and representing his doctrines, together with detailed histories of the principal cases which have been published, is contained in the monograph of MM. Bourneville and Guerard, which is the most complete work on the subject. It is to this work and to the lectures of Charcot that we are mainly indebted for the facts detailed in this notice. Since the publication of this treatise we have a case very carefully reported by M. Schüle, of Illenau, with a number of ingenious pathological observations on the disease.

There are three forms of multiple sclerosis—the spinal, in which the disease is confined to the spinal cord; the cerebral, in which the encephalon alone is affected; and the cerebro-spinal, by far the most frequent form, in which both brain and spinal cord are involved. It will be sufficient if we give an outline of the symptoms and course of the last-mentioned form, inasmuch as it is that of greatest importance, and as in it the symptoms of the more limited varieties are combined.

The symptoms may set in either abruptly or gradually, and they may in the beginning be referable either to the encephalon or the spinal cord. If the spinal symptoms are the first to manifest themselves, there are found heaviness, numbness, and formication, followed by loss of power in one or both lower extremities; after a time this condition comes to involve the upper extremities also. The paresis in some instances goes on progressively increasing; in others remissions occur, to be followed by exacerbations. The patient walks with difficulty and is easily overset. It is usual for these symptoms to continue for a long period before cerebral

phenomena manifest themselves. When these supervene there are weakness of sight, diplopia, cephalalgia, vertigo, and difficulty in articulation, together with transient attacks of an apoplectic character, in which consciousness is not lost. This constitutes the initial phase of the malady; after a variable period has elapsed tremors occur, affecting successively the lower and the upper limbs, the head, the globe of the eye, and the tongue. While the patient lies quietly no abnormal movements are observed, but when he attempts to raise his arm or his leg, the shaking begins, if he sits up the head is agitated with rhythmical movements. The globe of the eye is the subject of oscillatory movements when the patient desires to look steadily at an object. The articulation is at this time slow and embarrassed.

The shaking can also be brought on by any emotional disturbance, even without movements on the part of the patient. It varies in degree, and is sometimes so slight as to be only recognizable with difficulty. At an advanced period of the disease, when contractions set in, the trembling ceases. The paralysis of the affected limbs becomes gradually more complete, the patient can no longer stand or walk without assistance. After a variable period, usually five or six years from the commencement of the disease, the paralysed limbs become rigid or contracted. Spasms, sometimes tonic, sometimes clonic, show themselves in the lower extremities occasionally, but more rarely in the upper. The difficulty in articulation becomes greater; every syllable being followed by a pause. Vision becomes more impaired. The mental powers are much weakened. Sensibility remains, however, unaltered. Respiration, circulation, and the secretions are unaffected. The bowels usually require purgatives, otherwise digestion is normal.

In the advanced period of the disease the patient becomes quite unable to leave his bed. He can no longer articulate intelligibly. Deglutition becomes difficult and painful. Appetite is diminished, while thirst is increased. Nutrition is impaired, and the patient becomes emaciated. Diarrhoea often sets in. The mental powers become more and more enfeebled, and are finally almost abolished. The sphincters lose their power, and sloughs form on the sacrum.

The patient dies either from gradual exhaustion of the powers of life or from some intercurrent affection. Thus phthisis has become developed in four cases; in others death has been caused by bronchitis, pneumonia, pleurisy, erysipelas, dysentery, and apoplectiform attacks.

Anatomical characters.—Viewed with the naked eye, the spinal cord in a case of sclerosis is found to present a variable number of spots of a colour resembling that of the grey substance of the convolutions of the brain, but somewhat deeper in tint. On examining the surface of the brain it is not possible to distinguish the spots or patches of sclerosis from the healthy substance which surrounds them, owing to the change in colour being very slight; in this part of the nervous centres accordingly, as well as in the cerebellum, it is necessary to make sections, so as to be able to recognize the diseased parts. After being for some time exposed to the air the sclerosed patches acquire a salmon-red tint. Bouchard has pointed out that if the surface be brushed several times with an ammoniacal solution of carmine and then washed with water, the unaffected parts will be found to be uncoloured by the re-agent, while the spots of sclerosis will be found to have acquired a distinctly red colour. Placed in an aqueous solution of chromic acid, the sclerosed parts became yellowish, and subsequently white, while the healthy tissue assumes a greenish-grey colour.

The patches of sclerosis are firmer and more resistant than the surrounding tissue, and are also somewhat depressed below the level of the surface. In size they may exceed a square inch or be of the smallest dimensions recognizable. Their breadth is less than their length, and on section they are found to be irregularly conical. Their number is usually considerable, and they are scattered over the nervous centres, apparently without any attempt at order or regularity. In one case they have been only met with in the encephalon, in others they have been confined to the spinal cord, but in the majority of cases they have been formed in both the encephalon and the spinal cord. In some cases the process of sclerosis has been found to have involved the roots of some of the cranial nerves. When a spot of sclerosis is examined with the naked eye there seems to be a distinct line of demarcation between it and the surrounding parts, but this is only apparent for minute examination, shows that the neighbouring parts also have undergone alterations of a morbid character.

The essential histological characters of the morbid changes found in sclerosis may be conveniently described under two heads:—1stly, the delicate connective tissue lying between the elements of the true nerve-tissue is found to be increased in amount. This substance—the neuroglia—about which so much

discussion has taken place, is found to have undergone a hyperplastic process, which is evidenced by a multiplication of the nuclei contained in it, as well as by an increase in the fibrous element. A similar multiplication of nuclei is observed to have taken place in the coats of the minute vessels, and their lymphatic sheaths are filled with fatty granules. Amyloid corpuscles also are frequently found. Cholesterine has, in one case, it is stated, been met with. These changes, there is reason to believe, as we shall have occasion again to state, are the primary and essential characters of the disease.

2ndly, The nerve fibres are found to present very marked changes. In some places they are altogether destroyed, but this complete destruction seems to be unfrequent; more often what occurs is that the medullary sheath alone has been destroyed, but that the axis cylinder remains intact, and can be recognized by the colouration which it receives from carmine. This destruction of nerve fibre is owing to the compression exerted by the hypertrophied connective tissue.

The nerve-cells also undergo change. They become smaller and are filled with fine granulations, which give to the cell a dark appearance.

As regards the etiology and nature of the morbid process which brings about these pathological alterations, little is as yet definitely known. According to current pathological doctrines a formative irritation of this kind comes into the category of inflammatory processes. It is in the highest degree probable, as we have before observed, that the changes in the interstitial tissue constitute the primary lesion, and that the morbid alterations of the nerve fibres and cells are secondary to these and caused by them; so that pathologically sclerosis might be perhaps correctly designated as a "chronic interstitial encephalo-myelitis." But why the process assumes this form, what regulates the distribution of the spots of disease, and what is the cause of the diffusion of the peculiar process which constitutes sclerosis, are questions which we are quite unable to solve.

Nor is it at all easy to understand how it is that sensibility remains practically unimpaired in the midst of the complete wreck of the other nervous functions. In ten cases it has been noted that no anesthesia was present, while in three only was loss of sensibility noticed. In one hyperesthesia was observed. This question touches one of the most important and still unsettled points of spinal physiology.

It has been ingeniously suggested that some of the motor phenomena may depend on the loss of the medullary sheaths of the nerve-fibres, allowing the axis cylinders to become as it were uninsulated, so that a motor impulse, instead of being confined to the particular fibres along which it has been originally transmitted, becomes diffused, and comes to excite other fibres in contact with them, this diffusion being rendered possible by the axis cylinders being uncovered.

Diagnosis.—The spinal form of sclerosis is liable to be mistaken for locomotor ataxy. The points of distinction are that in the former there is real loss of power, which increases till it becomes an actual paralysis; while in the latter muscular power may be perfect, in the former there is no change produced in the gait or in the power of standing by closing the eyes, a test which distinguishes this malady at once from locomotor ataxy. There are other diagnostic marks in the history and progress of the different affections.

From paralysis agitans, with which it has been hitherto confounded, sclerosis may be recognized by the trembling only appearing when motion is attempted, by the peculiar embarrassment of articulation and by the nystagmus which are not observed in paralysis agitans. The mode of progression also is different in the two diseases, as well as the peculiar forms of contraction of the extremities, which are unlike each other in character and aspect.

Prognosis.—As we have before indicated, the progress of the disease has not hitherto been arrested by any remedial measure. All the cases hitherto observed have terminated fatally.

Treatment.—Various nervine remedies have been employed, and some with apparently temporary benefit. Nitrate of silver appeared to be useful in some cases. Strychnia and electricity seemed to benefit a patient of M. Piorry. Dr. Pennock thought that he derived some benefit from hydropathy, but as yet no remedy has been discovered which has produced lasting benefit in any case.

EPIDEMIC OF CHOREA.

An epidemic of chorea has been observed by Professor Steiner^a as occurring in Prague during the past winter. It has been several times observed that a number of cases of chorea are liable to occur at the same time in the neighbourhood of a patient suffering

^a *Jahrbuch für Kinderheilkunde and Physische Erziehung.* Heft. 3. June 10th, 1870.

from this affection, and undoubtedly from imitation. The cases observed by Steiner, however, occurred in different quarters of the city, some outside the city altogether, and under circumstances as which precluded the possibility of imitation having anything to do with their production. Between the 15th of January and the end of February nineteen cases were noted by the author of this paper. All were in children, and eighteen of the patients were females.

As regards the cause of this outbreak of chorea, Steiner believes that it is to be found in the severity of the prevailing weather, and in the rapid fluctuations of temperature which then occurred. In five of the cases rheumatic evidences were found. It was observed that at the same time influenza and rheumatic affections were prevalent. These facts, coupled with the simultaneous development of the choreic symptoms in so many cases, led to the opinion that the exciting cause of the malady was to be found in atmospheric influences. Out of fifty-two cases of chorea previously observed by Steiner, eighteen had occurred during the month of January.

As regards treatment, bromide of potassium was freely tried, but was not found satisfactory. The best remedy was found to be Fowler's solution, in doses of from two to three drops daily, increased by a drop every second or third day, till in some cases seven or eight drops were administered. When improvement commenced the quantity was gradually diminished. When the agitation was considerable, especially when it continued during sleep, laudanum was combined with the arsenic in the following proportion:—

Fowler's solution, eight drops.

Tincture of opium, six drops.

Distilled water, four ounces. Mix.

Of this four dessert spoonfuls were given daily. It was very rarely found that symptoms of the physiological effects of arsenic were produced.

PROGNOSIS IN CHRONIC DISEASES OF THE HEART.

An important paper on this subject was read before the Medical Society of the county of New York, by Dr. Austin Flint, in March of the present year, and will be found in the *New York Medical Journal* for May. It discusses a subject about which there is still much misunderstanding, and the following remarks

will be found to be worthy of the high reputation of Dr. Flint as an authority on the subject of cardiac disease :—

“An important aspect under which the prognosis of chronic diseases of the heart is to be considered is the innocuousness of certain lesions. Lesions of the valves, as is well known, are represented by adventitious sounds known as endocardial murmurs. By means of these murmurs the existence of valvular lesions is determined, and they are readily localized. If there be found, in any case, endocardial murmur or murmurs persisting, and not due to a morbid condition of the blood, we have the proof of a chronic structural affection; there is organic disease of the heart. But the lesions which give rise to murmurs are by no means always of importance as regards immediate or even remote evil consequences. They may be devoid, not only of danger, but of any morbid symptoms. There are many persons pursuing their various avocations, and wholly unconscious of any malady, who, if auscultated, would be found to have organic disease of the heart. In a certain proportion of these persons the existence of cardiac disease will hereafter be manifested by symptoms and morbid effects; some may at length die from the disease, but in not a few, even if life continue for many years, the only evidence of the disease will be, as now, the presence of one or more of the cardiac murmurs, and death will be caused by some affection which has no connexion with the lesions existing in the heart. In cases of innocuous lesions the harm of physical diagnosis is sometimes apparent. Let the simple statement be made authoritatively to one having an innocuous lesion that he has an organic disease of the heart, and he will be likely to look upon himself as doomed. If he be a timid, nervous man, he has received a moral blow from which he does not recover. He sees a sword suspended over him. He is under sentence of death. Not only is he hurt as regards his comfort and happiness, but the depressing effect of the diagnosis, and the altered habits of life to which it may lead, sometimes contribute to impair health, and tend, perhaps, to shorten life.

“I would not for an instant have it supposed that I mean to disparage physical diagnosis. I wish only to place in a strong light the importance of going further than to the fact of the existence of organic disease of the heart. In other words, I would prepare the way for saying that, with reference to the prognosis, more information than the murmurs can furnish is indispensable. What has just been said concerning the long-continued innocuousness of cardiac lesions, I may add, is warranted by my own observations. I have records of cases in which organic endocardial murmurs existed from ten to thirty years ago, the persons now living, and exempt from ailments referrible to disease of the heart.

"It is difficult, without the lessons of clinical experience, to appreciate the fact that the intensity and quality of heart-murmurs are not of much account in judging of the importance of valvular lesions. A murmur very loud, notably rough or musical, it would seem, should denote graver lesions than one which is feeble, soft, and blowing. Experience, however, shows that it is not so. A striking illustration of this fact came under my observation some time since. A gentleman from Cuba consulted me for disease of the heart. He had a musical murmur loud enough to be heard with the ear removed some inches from the chest. The sound had attracted his attention, and this first led him to see a physician. He was told that he had disease of the heart, of which he had previously had no suspicion, having no ailments referrible to that organ, and, indeed, considering himself perfectly well. He became at once a medical curiosity, and he had been examined by many physicians. The case exemplified the fact that the diagnosis of a cardiac lesion is sometimes a misfortune. The man had no peace of mind after the discovery of the murmur. He relinquished his business, and came to this country for medical opinions. The lesion, as regards present importance, was innocuous; and had he remained ignorant of its existence, he would not only have been contented and comfortable, but his condition would probably have been more favourable for the preservation of health.

"It follows, from what has been said, that, with reference to prognosis, it is important to go further in diagnosis than to determine, from the presence of murmur, the existence of an organic disease of the heart. If we except the accident of embolism, we are warranted in saying that, as a rule, in cases of valvular lesions giving rise to murmurs, whatever be their number, intensity, and quality, there is no danger, either immediate or near at hand, so long as the heart is not enlarged; for clinical observation shows that, in general, valvular lesions cause enlargement of the heart before leading to more remote effects which involve distress and jeopardize life. Moreover, clinical observation shows that in most cases enlargement of the heart is produced by valvular lesions slowly, the ulterior effects being, of course, proportionately distant. I would remark, in this connexion, that, in order to judge of the import of organic murmurs, aside from enlargement of the heart, the heart-sounds claim more attention from stethoscopists than is usually given to them. It is certain that the aortic and the pulmonic second sound can generally be interrogated separately by auscultation; and I believe this statement may also be made with respect to the mitral and the tricuspid valvular element of the first sound. The absence of any abnormal modifications of these several components of the two sounds of the heart is an important point in judging of the innocuousness of valvular lesions, the existence of which is revealed by the presence of murmur. . . .

"The difference in the tolerance of chronic affections of the heart is to be considered with reference to the prognosis. What is true of most chronic diseases, namely, that the same lesions are tolerated very differently in different cases, is especially exemplified by the structural affections of the heart. It is truly astonishing how well borne, in some cases, are cardiac lesions of unusual magnitude. A case which recently came under my observation afforded a striking illustration of this fact. The patient, a man of middle age, was suffering greatly from dyspnoea in paroxysms, together with loss of appetite and general prostration, and the case ended fatally within a few weeks after the occurrence of the symptoms just named. I saw the patient a few days before his death, and found the heart enormously enlarged. The apex-beat was in the eighth intercostal space several inches without the linea mammalis; and the dulness on percussion over the precordia was proportionately increased both in area and degree. Here was truly a *cor bovinum*. There were present murmurs, indicating both aortic and mitral lesions. There had occurred an attack of acute articular rheumatism fifteen years ago. Now, prior to a few weeks before death, this patient had seemed to be in excellent health, and he declared that he was so. He was a man of very active habits, engaged in a business (that of a wool merchant in the country) which required much travelling. He had had, on one occasion, an attack of hemiplegia, of very brief duration, which was probably attributable to embolism. With this exception, he had not for many years been a patient, considering himself a healthy man. He was a man of temperate habits, but a good liver as regards diet, eating very heartily, and digesting his abundant meals without difficulty; yet it is certain that for several years there must have been very great enlargement of the heart, resulting from the valvular lesions. For some time before the occurrence of grave symptoms referrible to the heart, he had had an unusual amount of mental and physical work, accompanied with much excitement; nervous asthenia and impaired appetite ensued, and, under these circumstances, he began to suffer from dyspnoea. He was compelled to keep the bed; he became despondent; the existence of disease of the heart was forced upon his attention, and he failed rapidly. The history of this case represents what I have repeatedly been led to observe in other cases, to wit, the tolerance of disease of the heart, while it was advancing, more or less slowly, until it had attained to a great amount, the person affected, in the meantime, not considering himself an invalid, taking no remedies, living freely, and engaged in pursuits involving activity of mind, or of body, or of both. The case also represents a fact which I have repeatedly observed, namely, that from the time when persons with disease of the heart become patients, that is, when they become impressed with a knowledge of the existence of the disease, and are obliged to give up their usual pursuits and habits, they are apt to

fail rapidly. It is a *facilis descensus* from that time. The latter fact, as well as the remarkable tolerance of the disease under the circumstances stated, teaches an instructive practical lesson.

"In speaking now of the tolerance of cardiac lesions, I do not, of course, have any reference to those which have already been referred to as innocuous. I refer to lesions which are more or less serious, that is, involving either obstruction to the free passage of blood through the orifices of the heart, or regurgitation, or both these immediate effects combined, together with enlargement by hypertrophy or dilatation separately or in combination.

"All clinical observers who have seen much of diseases of the heart must have been struck with the fact that the inconvenience and suffering attendant on lesions the same in character and extent, differ widely in different cases.

"What are the circumstances on which this variation as regards tolerance depends? This question not only has a bearing on the prognosis, but it is of great importance in relation to management. I will devote to it a few remarks.

"In general terms, chronic diseases of the heart, as of other organs, are tolerated in proportion as the functions of the body, exclusive of the part diseased, are healthfully performed. The internal conditions of general health and constitutional strength relate especially to the series of functions which begin with ingestion and end with nutrition. Other things being equal, the toleration is best and longest when, *first* of all, the ingesta are ample; *second*, when digestion is active; *third*, when, owing to adequate assimilation, the constituents of the blood are in normal proportion; *fourth*, when the nutritive supplies in the blood are well appropriated; and, *lastly*, when the secretory and excretory organs do their proper work. Now, a healthful performance of these functions is not incompatible with considerable damage of the central organ of the circulation; and, in so far as it is practicable to maintain these functions at, or near to, the state of health, the toleration of diseases of the heart will approximate to completeness. *Per contra*, the toleration will be incomplete in proportion as the functions of the body, exclusive of the heart, are feebly or imperfectly performed; in other words, in so far as the conditions just named of general health and constitutional strength are deficient. The blood may be considered as representing the healthful performance, or otherwise, of the functions of nutritive and destructive assimilation; so that the simple phrase, *healthy blood*, comprehends the grand requirements for toleration."

In the discussion which followed Dr. Flint's paper, the following statistics, based on ninety *post mortem* examinations made in the Bellevue Hospital, New York, were brought forward by Dr.

Alfred L. Loomis. They illustrate the question of how often and in what manner cardiac lesions are the direct cause of death:—

“It will be seen that valvular disease, cardiac hypertrophy, and dilatation were present in fourteen cases. Of this number, heart-lesions were the cause of death in seven; death was sudden in one, and was caused by stenosis of the mitral and tricuspid orifices.

“In fifteen cases valvular lesions with cardiac hypertrophy were present, in eleven of which the heart-lesions were the cause of death; in five of these death was sudden, and the valvular lesions were aortic in one, mitral in another, aortic and mitral in another, mitral and tricuspid in another, and mitral and pulmonic in another.

“In six cases valvular lesions with cardiac dilatation were present. In four of these the heart-lesions were the cause of death; two died suddenly, in one the valvular lesions were mitral stenosis and aortic thickening, in the other the aortic, mitral, and tricuspid valves were all diseased.

“In forty-six cases valvular lesions were present without cardiac hypertrophy or dilatation. In only two of these were heart-lesions the cause of death, in neither of which was death sudden. Lesions of the coronary arteries were present in three cases; in one death was sudden. Thrombi of the heart were present in six cases, death sudden in one.

“It will also be seen that the number of deaths due directly to heart-lesions was twenty-six. In nineteen cases death was sudden; number of sudden deaths due to heart-lesions, ten; number of gradual deaths due to heart-lesions, sixteen; number of deaths not due to heart-lesions, sixty-four. Of the nine sudden deaths not due to heart-lesion, four were from cerebral apoplexy, four from uræmic convulsions, and one from croupous laryngitis.”

REPORT ON OBSTETRIC MEDICINE AND SURGERY.*

By GEORGE H. KIDD, M.D., F.R.C.S.I., L.K. & Q.C.P.; Obstetric Surgeon to the Coombe Lying-in Hospital; Vice-Pres. Obstet. Soc., Ireland; Cor. Mem. of the Gynecological Soc. of Boston.

DISEASE OF THE OVARIES.

SPENCER WELLS^b and KEITH^c have both given us further accounts of their experience of ovariectomy. Mr. Wells' tables are not yet complete; but, in the meantime, the following paragraph from his introductory remarks is of much interest, and we hope soon to have his explanation of the facts:—

“After looking over the table we will consider what lessons we may learn from the facts so recorded, but I may premise that the results in hospital have not been nearly so satisfactory of late as in private. In my early experience the advantage was on the side of the hospital patients. Then the results became equalized. But latterly the advantage has been greatly on the side of the private patients. Thus, in these tables I have particulars of 47 cases operated on during the last two years. Of them 28 recovered and 19 died, a mortality of 40 per cent. But during the same period in private practice I have operated on 57 patients with a result of 43 recoveries and only 14 deaths—a mortality of 24 per cent., which, compared with the 40 per cent. in hospital, is a startling difference. The causes of this difference we will investigate, after carefully arranging the facts.”

Mr. Keith records a second series of 50 cases, making a total of 100. None of these were treated in hospital, for, as he says, “I hold no hospital appointment;” but 70 of the cases were treated in the same room, of whom 60 recovered, and nearly all the worst operations were performed there. The greater number of those who died were poor worn out women, who came late in the disease. The mortality, Mr. Keith remarks, would probably have been much lower if there had been earlier operations in many of the cases, “not that small tumours, giving little trouble, should be

* The author of this Report, anxious that every contribution to Obstetric Medicine and Surgery should be noticed, will be glad to receive publications on the subject. They may be sent to the publishers of the Journal through their correspondents.

^b Medical Times and Gazette, September 3, 1870—“Hospital Experience of Ovariectomy.”

^c Lancet, August 20, 1870—“Second Series of Fifty Cases of Ovariectomy.”

removed; for, on the whole, cases do best where the tumours are large and the general health somewhat impaired. There is, however, a stage in the progress of almost every case of ovarian disease in which the operation is safe; and if this favourable time be allowed to pass, no care or skill can make up for it."

Of the total 100 operations there have been 81 recoveries, and of the last 50 there has been a gain of 6 per cent., while those who died survived the operation longer. In the first series death took place on the average on the third day, and in the latter during the tenth. The operations were quite as severe as in the first series of cases, many of the patients were badly constitutioned, and not a few had sought relief elsewhere in vain. The mortality in the latter series was increased by the occurrence of four cases in which the tumours were of a malignant character. In the first series of 50 cases the mortality was 22 per cent.; in the present series it was only 16 per cent. In three cases the operation, though commenced, was not completed, and no harm was done. In the first of these, on drawing out the cyst, the posterior adhesions were found so old and intimate that nothing more was attempted. A free opening was left in the cyst; the patient got into very good health, married, and remained well for two years, when the fluid rapidly collected, and tapping had to be performed. In the second a very firmly-adherent thin cyst was simply emptied. The patient recovered perfectly, and was afterwards treated by drainage. In the third the tumour was so fused with the uterus and bladder that nothing was done but to empty two cysts. The patient recovered as after an ordinary tapping, and as yet continues well.

Dr. Keith gives the notes of 16 cases in which no operation was performed, in most of these he declined to operate, either from the patients being in too debilitated a state, or from the adhesions being evidently too extensive to allow of the removal of the tumour.

In one of the fatal cases the operation was looked on as simple and safe, but the patient died from pulmonary embolism fifteen days after the operation. Two died from pyemia, in one of whom a part of a very adherent tumour was left attached to the sacrum, in the other the tumour was cancerous and adherent to the rectum. One fair average case, in which both ovaries were removed, died from a large fibrinous clot in the right auricle, without any morbid appearance being found in the abdomen. One case of malignant tumour seemed to be doing fairly well when rapid pulmonary congestion carried her off on the sixth day. Another of the malignant

cases got over the immediate effects of the operation, and lingered on for 23 days, worn out, apparently, by the general cancerous disease. In one, suffering from chronic pleurisy and ascites, so much relief had followed thoracentesis that ovariectomy seemed justifiable. On the fifth day after the operation the pleura again rapidly filled; thoracentesis was again had recourse to, but it did not save her, while the very small amount of healthy lung found after death showed, says Mr. Keith, that there had been an error of judgment in doing ovariectomy at all.

In one case Mr. Keith attributed death to the excessive vomiting from the chloroform, and he has since entirely abandoned the use of this agent in ovariectomy and other severe and tedious operations, and now always uses anhydrous sulphuric ether, made from methylated alcohol, administered with Dr. Richardson's apparatus. This, he says, causes infinitely less vomiting, and instead of the pallid face and feeble pulse of chloroform, the patient, after a long operation, is put to bed with a flushed face and great surface circulation. Mr. Keith has now used sulphuric ether (at first with a small proportion of chloroform) in 53 operations, of which 46 recovered.

When practicable, the extra peritoneal method of treating the pedicle was adopted, and Mr. Keith thinks greater success will probably attend this than any other single method. In 40 clamp cases there were three deaths, but these were in no way connected with the mode of treatment adopted. Ligatures on the pedicle have also once or twice been cut short and returned; sometimes they have been left hanging out at the wound, and of late Mr. Keith used the cautery several times with excellent results. Each method, Mr. Keith thinks, has advantages in certain cases.

With respect to the anesthetic used, it may be observed that of the ten cases already published in Mr. Well's present report, bi-chloride of methylene was used in five and chloromethyl in the remainder.

In connexion with this subject the following case, translated in the *Edinburgh Medical Journal* for March, 1870, from the *Gazette des Hôpitaux*, is of such interest as to seem worthy of being quoted in full:—

“REMOVAL OF UTERUS AND ITS APPENDAGES.—The following short notice of a remarkable operation, performed by M. Pean, the subject of which was shown to the Academy of Medicine in Paris, at its meeting on December 8, 1869, is from the pen of Dr. Revillout:—

"The tumour is multiple, containing, 1. A very large cyst of the left ovary. 2. The uterus itself hypertrophied, containing a cyst, and extending up as far as the umbilicus. 3. A fibrous tumour of the right ovary, and a cyst of the right Fallopian tube. . . . Before the operation, M. Pean had diagnosed the existence of a movable fibrous tumour, independent of the ovarian cyst. He had also made out by vaginal examination that the uterus was hypertrophied, but he did not know to what amount, and could not make this out on account of the adhesions which bound the tumours to each other.

"When the cyst of the left ovary became visible through the incision in the abdominal wall, M. Pean began to remove it in his usual manner, stopping the hemorrhage by long cauteries, heated to whiteness. After having got out a large part of the cyst, he arrived at the uterus, which he found to be greatly enlarged, soft, fluctuating, and everywhere adherent. These adhesions were very vascular, and could not be broken down without excessive hemorrhage, which even the cauteries failed to arrest. In such circumstances, no resource remained but to remove the diseased uterus along with the other tumours. This M. Pean did not wish to do, through the neck of the uterus, as it, excessively hypertrophied, was as large as a fist. He preferred to carry the section through the vagina itself, and he succeeded in passing across the vagina from before backwards, and through the abdominal wound, a double thread, by which he tied the tumour in two halves; the one on the left side secured the great ovarian cyst, the other one included all the vagina corresponding to the uterus, and isolated, besides that organ, the right ovary and Fallopian tube. After having tied the two ligatures, M. Pean cut off all the tumours close above them, and then by strong pulling brought the pedicle (such as it was) to the abdominal wound. This he did not close at a point opposite to the pedicle. Three india-rubber tubes were passed down to the bottom of the cyst to allow escape of discharge. A good recovery took place."—*Gazette des Hôpitaux*, No. 143, 1869.

Fluctuations in the size of ovarian tumours are very rarely observed, but in the following case, brought before the Edinburgh Obstetrical Society by Dr. Ritchie,^a they were very remarkable. The patient was a Miss M. O., who was placed in a private lunatic asylum in 1856, being then in her 38th year of age. In March, 1866, her size attracted attention, and she was placed in the infirmary of the asylum, and remained there under the care of Dr. Millar till she died. Dr. Ritchie has placed in a tabular form the measurement recorded by Dr. Millar, which, with some of his observations on the case, are as follows:—

^a Edinburgh Medical Journal, March, 1870, p. 849.

In circumference.			
1867.	April,	= 54	inches, maximum.
„	19th May,	= 51	„ minus 3
„	19th Sept.,	= 41	„ — 10
„	19th Oct.,	= 38	„ — 3
„	5th Nov.,	= 40	„ plus 2
1868.	23rd Feb.,	= 45	„ + 5
„	26th June,	= 49	„ + 4
„	25th July,	= 35	„ minus — 14 minimum.
1869.	March,	= 45	„ plus 10
„	April,	= 47	„ + 2
„	May,	= 48	„ + 1
„	June }	= 50	„ + 2
„	July }		
„	Aug.,	= 51	„ + 1
„	Sept.,	= 51½	„ + ½
„	23rd Nov.,	= 50½	„ <i>post mortem</i> circumference.

“In its obstetrical aspect he thought the case was also interesting. The first distinct notice of her true condition was in March, 1866, but long previous to that date her peculiar appearance had attracted notice, and it was not improbable she had been self-conscious of it, as she used frequently to say she was in the “family way.” In April, 1867, her condition was one of great urgency, which seems to have been connected with the tumour, and it gave a starting point as to her size. An interesting point, too, was that her state at this time apparently indicated the commencement of her first reduction in size, for during the next month she became 3 inches less, and in September there was a more rapid decrease of 10 inches; with the reduction in the following month, there was consequently a decrease in the course of five months of 16 inches; and it was of importance to note that in September, when the rapid decrease took place, the only condition noticed was that she passed immense quantities of water. Had there been any injury to the cyst, some constitutional disturbance would likely have been produced. For eight months she went on increasing in size, till on 26th June she had gained the girth of 49 inches, or 11 more than in October. The second decrease occurred in July, and was very rapid, amounting to 14 inches in the course of a month; and from this time till her death, sixteen months after, she steadily gained in size, but never reached the girth when first measured in April, 1867.

“This diminution in circumference having occurred twice, might, he remarked, form the subject of considerable speculation, and that various theories to explain it might be advanced. The first point to settle was, whether there was more cysts than one. At first he was inclined to suppose there had been, and that one had ruptured on each of the two occasions alluded to; but after making a minute examination of the

preparation, he could not satisfy himself as to there being any traces of a ruptured cyst, for he considered the cysts must have been of great size when they ruptured, the decrease being in the one instance 16, and in the other 14 inches. He felt satisfied the small thickened portion of the preparation could not represent a collapsed cyst of the required size. His speculations being limited, therefore, to the existing cyst, he arrived at the conclusion, that, although there were numerous traces of rudimentary cysts, one only had developed to great size. The next question which occurred was, how did the fluid escape from the cyst? He could detect no evidence of fistulous opening into the genital tract; and had there been one into the intestinal canal, it would not have been overlooked by Dr. Millar. After several attempts, a piece of fine whalebone was passed along the right Fallopian tube. Supposing that rupture had occurred, there being no trace of fistulous opening, and only the right Fallopian tube being pervious, the matter was reduced to this, could a tube of so small a diameter, that an ordinary probe could not be passed along it, and in which there was no evidence of previous dilatation, permit of such a flow of fluid along it as to cause the rapid diminution which took place, especially on the last of the two occasions? His opinion was, it could not. He could see no other way in which it could be removed but by the kidney. He referred to the observations of Sir J. Y. Simpson, Hewitt, &c., on the removal of ovarian fluid from the peritoneal cavity, but remarked that their observations were not applicable to the case before the Society, for in it there was no account of such a train of symptoms as accompany the sudden rupture of a large ovarian cyst, and the extravasation of its contents into the peritoneal cavity; and what was even more important, there was no trace in the walls of the cyst of rupture having occurred. Dr. Thomas Keith had kindly examined the preparation, and was also of opinion that no rupture had occurred. He had seen several cases of a somewhat similar kind to that of Miss M. O., and had observed decrease of size, and its subsequent increase. The conclusion Dr. Ritchie came to was, that the fluid had been in some way absorbed from the cyst directly, and without the intervention of rupture. In the cases in which Dr. Keith had observed a similar decrease, the fluid, as in Miss M. O.'s case, was clear and thin. Dr. Ritchie thought it was possible, and he might even say probable, that an endosmotic change had taken place by means of the large vessels, to which allusion was made, among the *post mortem* appearances. This was favoured by the extreme thinness of the posterior wall of the cyst, which was quite transparent, and by the limpid character and low specific gravity of the contained fluid. In such a view, it would be expected that when the distention of the cyst was greatest, the walls therefore thinnest, and the surrounding blood-vessels most stretched, that the transmission of the fluid would most likely commence; and referring to the case, it would be observed the decrease in size in the first instance

began when the greatest distention had been attained, and the increase began when the reduction had reached 16 inches. In the second instance the reduction commenced when the circumference had reached 49 inches; and when the lowest measurement of 35 inches was arrived at, the cyst reversed the action, and again filled. It would be instructive to have his view, on the one hand, confirmed, or, on the other, to have a more satisfactory explanation of the phenomena. He had only further to add, he understood Dr. Keith to say he had rarely seen so small a pedicle, and that the case would have been an excellent one for operation."

An Aid to Parturition and to the Treatment of Displacements of the Uterus by a new Mechanical Appliance.

Under this title Dr. Protheroe Smith^a has contributed to the Edinburgh Obstetrical Society a description of an ingenious instrument which he exhibited at the meeting of the British Medical Association last year at Leeds, and again this year, with certain improvements, at Newcastle.

As an aid to parturition, the instrument is intended to aid or supply the place of the voluntary muscles which assist the uterus to expel the child in the second stage of labour; and Dr. Smith, though he does not seem to confine its use to such cases, believes that it will be found specially useful when the muscles have lost power from the degeneration caused by injuriously tight clothing, or when their action is suspended by the use of anesthetics.

In a further communication^b Dr. Smith refers to Dr. Haughton's researches on the force exerted by the muscles in parturition, published in the May number of this Journal, and adopting Dr. Haughton's conclusions, that while the pressure exerted by the involuntary muscles is only 54·1 lb., and that of the voluntary muscles is equal to 523·65 lbs. he claims that where this voluntary power is deficient his instrument will supply its place.

The instrument consists of steel bands clasping the pelvis below the cristæ ilii, having a sacral and a pubic pad. From the sacral pad two bars or springs pass upwards, one on each side of the spinal column as far as the scapulæ, and terminate in a band that surrounds the thorax either at its upper or lower margin. About the middle of the lumbar region these upright bars have a short horizontal bar attached to them, to the extremities of which a belt to surround the abdomen is attached. This belt is sufficiently broad

^a Lancet, June 4, 1870, p. 798.

^b Edinburgh Medical Journal, April, 1870, p. 885.

anteriorly to cover the whole of the front of the abdomen, and has running straps and buckles by which it can be tightened in segments from above downwards. It is also connected by elastic straps that pass downwards to the pelvic band. During labour the segments of this band are tightened in succession from above downwards, the downward action being assisted by the elastic bands, the intention being to compress the fundus of the uterus and so extrude the child. The bands may be tightened during the pain, and relaxed during the intervals, or be permanently tightened till the uterus is emptied, and may be used to compress the uterus to prevent post-partum hemorrhage.

It does not seem likely that this instrument will come into general use. Fortunately the abdominal muscles are seldom in such a state of degeneration in child-bearing women as to require such artificial aid, and even the most zealous advocates of anaesthetics do not give them in ordinary labours to such an extent as to suspend the action of the voluntary muscles. That in skilful hands the instrument may be used with safety is proved by the cases published by Dr. Smith, but it must be liable, when not so carefully used, to produce contusions of the abdominal muscles, and of the uterus itself, and possibly to set up an irregular action of the uterine fibres, and so cause inversion of this organ. The expense of the instrument is another objection to its use, though of course a minor one.

Dr. Smith also claims for this instrument that it is a valuable agent for the mechanical treatment of uterine ailments. He argues that the spine, by its lumbar curve, maintains the pelvis in an oblique position, which is necessary for the support of the pelvic viscera. This curve is often lost from defective muscular power, and may be maintained by mechanical aid. On this part of the subject Dr. Smith promises a further communication, but in the meantime the following passage from the paper in the *Edinburgh Medical Journal* may be taken as indicating his views:—

“Man being erect, it necessarily follows that the structure of the pelvis and abdominal muscles must be such as to retain the viscera in their proper position, and to make the necessary expulsive efforts, which defecation and parturition require, with impunity.

“The spinal column thrown into a series of waves, not only lessens shock from concussion, but by its lumbar curve the plane of the pelvis is rendered oblique, constituting that graceful form which, when absent, is artificially supplied by a pad or bustle, and which is so designed as to

support the abdominal viscera on the pubes, aided by the abdominal muscular walls and spine, and to retain them within the pelvis. But when the plane of the pelvis becomes horizontal by the obliteration of the lumbo-spinal curvature, or, as it is called, the Grecian bend, the abdominal viscera tend to prolapse towards or beyond its outlet, and to displace the pelvic contents.”

“For the mechanical treatment of uterine ailments, this instrument acts in the following way:—The pubic and sacral pads fixed by the lateral connecting springs tend, by their mutual re-action, to alter the plane of the pelvis, if too horizontal, to one more oblique, and therefore more natural. The costal springs and sternal pad, attached to the end of the perpendicular springs, help still further to increase the lumbar pressure, to throw the sacrum backward, and, at the same time, to fix the whole apparatus. From the pubic pad, and attached to it by a movable rack-work, is a curved steel spring, which, bent to the shape of the vagina, is passed into it, whilst it is made to carry at its extremities any form of pessary or support that may be required.”

This will probably be found the most useful and valuable application of the instrument; but Dr. Smith does not seem to have been aware that he was preceded in the recommendation of it by an American physician, Dr. Banning, who described a very similar instrument to the Section on Obstetrics of the New York Academy of Medicine, on 21st May, 1866, in a paper afterwards published in the *Medical and Surgical Reporter* of Philadelphia, on 16th of June, 1866. In the following passage Dr. Banning explains his views; and though it may be that he claimed too much for his “Abdominal and Spinal Shoulder Brace,” and thus prevented its attracting the attention it deserved, it is to be hoped Dr. Smith’s papers will now obtain for the method of treatment a full and careful consideration:—

“OF THE MECHANICAL PATHOLOGY OF UTERINE OBLIQUITIES.

“From much observation, I am induced to think that the prevalent pathology of uterine obliquities is more or less defective, the ruling idea of it seeming to be, that the physical causes originate within, and are mainly confined to the internal pelvic tissues; whereas, to the writer, it seems manifest that, in the premises, the pelvic contents are, in the main, only the objective point, and that the abnormal status there, both primarily and proximately, is caused more or less by a relaxation of the abdominal muscles and ligaments, and by a consequent undue gravitation, not only of the abdominal contents upon the pelvic organs, but also of the whole trunk, which has lost its true centripetal bearings, and has fallen

forward of its spinal axis, in consequence of a diminished and unbalanced action of its muscular braces.

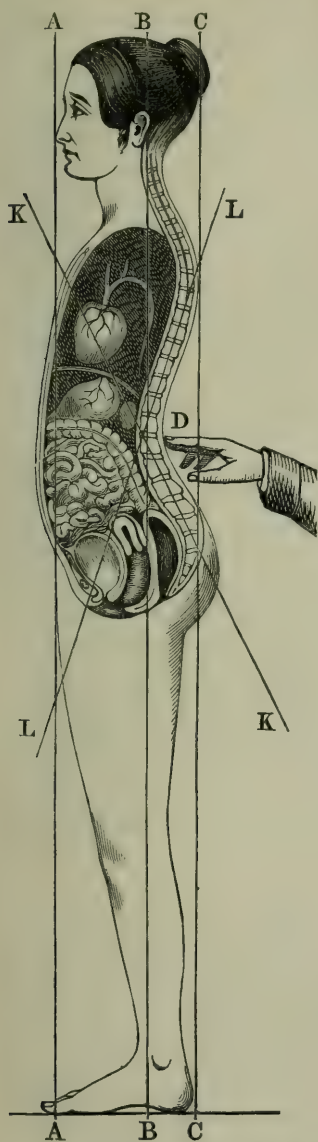


Fig. 1.—Side view of erect posture, with natural upward and inward bearing of the internal organs.

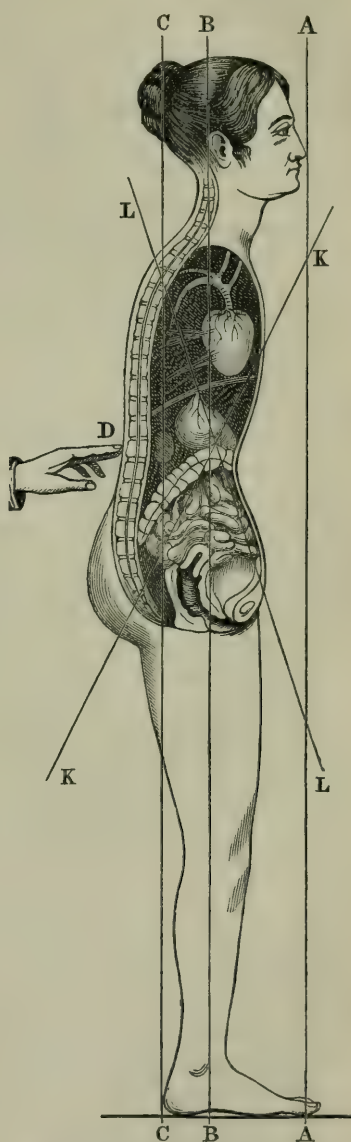


Fig. 2.—Side view of drooping posture, with internal organs suspended and compressed.

"To illustrate: at a mere glance at Fig. 1, we see plainly that the mathematical combinations of such a figure produce a *centripetal* state of all the trunkal bearings, or, in other words, a balancing of the superior trunk upon and behind the spinal axis (or *point d'appui*), a tension of all the abdominal muscles, a consequent expansion of the chest, and a protection of the pelvic viscera from superincumbent abdominal weight, by a steady maintenance of the whole visceral series in the ascendant. Add to this the fact that, in such a figure, the medial plane of the pelvis is rendered comparatively vertical, and the lower abdominal cavity correspondingly small antero-posteriorly. By this combination, not only is the descending weight of the viscera impeded, but also the force of visceral gravity is compelled to fall upon the pubes, and not upon the uterus, rectum, and bladder, in the direction of the inferior strait.

"On the other hand, a glance at Fig. 2 shows, almost painfully, that a *centrifugal* state *reveals*, as it were, throughout; for see the spine has retreated *behind* the body, leaving the whole trunk to hang forward *from* the spine, and not to swing *behind*, or to rest upon it, as in Fig. 1.

"This state causes the chest to droop, the ensiform cartilage to retreat towards the spine, the medial plane of the pelvis to become horizontal, like a dish, the distance between the sternum and symphysis pubes to be much diminished, and the abdominal muscles to become flabbed; also, the inferior abdominal cavity becomes greatly enlarged antero-posteriorly, and the head, shoulders, and visceral series descend, and of consequence, press with corresponding force upon the uterus, bladder, and rectum.

"The contrast between the two is complete; and, whatever *inherent* causes there may be to produce uterine obliquity, it is not *certain* that such a condition of the internal, middle, and superior trunk, as is represented by Fig. 2, must greatly augment the evils, and remain at least an *obstacle* to a complete curative action; and is it not also evident, that this undue pressure must be greatly augmented by the superinduced horizontal state of the medial plane of the pelvis.

"CURATIVE INDICATIONS.

"The idea of uterine obliquity being caused or aggravated by superincumbent *trunkal* weight being conceded, light at once breaks, as to some of the indications of cure. First, we should remove the superadded burdens from the uterus and its ligaments, by restoring the body to its normal or centripetal bearings, as in Fig. 1, by pushing forward the *point d'appui*, or dorso-lumbar portion of the spine, to an axial line between the ankle and the head. Thus we immediately restore the normal philosophical bearings of the skeleton trunk. (See mathematical diagram, Fig. 1, compared with that of Fig. 2.) For by thrusting this portion of the spine forward into the vertical axis of the body, the very weight of the head and shoulders becomes an *elevating* agent, a tensor of

the abdominal muscles, and a consequent contractor of the inferior abdominal cavity, by being compelled to throw its gravity *behind* the spinal fulcrum. This also has compelled the upper sacrum to advance, the symphysis pubes to correspondingly depress and retreat, and thereby restore the normal pelvic obliquity, which shelters the pelvic organs in the inferior strait *below* and *behind* the upper sacrum, and compels the pubes and lower abdominal muscles to receive the principal abdominal weight, which is here supposed to be so burdensome to the uterine ligaments. This balanced state of the trunk upon its fulcrum, and elevated state of the viscera once permanently accomplished, whether by nature or art, the case is changed from that of a general trunkal derangement to a more local one, and the inherent and artificial resources are left to contend only with the inconsiderable weight of the uterus."

The objections to Dr. Smith's instrument for applying pressure to the uterus as an aid to labour do not, however, apply to the method recommended, according to Dr. Playfair, by Von Ritzen, in 1856, and carried into practice by Kristeller, in 1857. Dr. Playfair^a details two cases in which he adopted the practice, and gives the following description of it:—

"The object was, to *push* the presenting part through the pelvic canal in cases in which the forceps would otherwise be required to *pull* it through; to apply, in fact, a *vis a tergo* instead of a *vis a fronte*.

"This proposal has met with but little attention in this country; and the only author who, as far as I know, refers to it, is Dr. Barnes, in his recent admirable work. He says with regard to it: "This resource, then, should not be lost sight of. In certain cases it may obviate the necessity of using the forceps; or it may stand you in good stead when instruments are not at hand.

"It is certain that the advantages to be derived from external pressure are not yet widely known or recognized; and as I have now received very material assistance from it in many cases of lingering and powerless labour, I believe it may not be without interest to state briefly the result of my experience on this point, especially as I do not know of any published cases in this country in which its use has been described.

"The class of case in which external pressure is likely to prove serviceable is of very frequent occurrence—viz., in which the presentation is natural, and the pelvis roomy, but in which delivery is retarded simply from deficiency or absence of uterine contraction. These are the cases in which resort to the forceps is so often essential, in which the head has passed well into the pelvis, possibly descended as low as the perineum,

^a Lancet, October 1st, 1870, p. 465.

and in which apparently but one or two good pains are required to complete the delivery.

"Firm pressure, applied under such circumstances, may act in two ways: First, and most commonly, it may merely stimulate the sluggish uterus to increased exertion, just as firm pressure after delivery will cause a relaxed uterus to contract. In this way pains that are feeble and ineffective may be rendered strong and useful, and a natural termination may result when artificial assistance might otherwise be required. I have of late been frequently in the habit of thus stimulating the uterus, and I feel certain that I have in many instances greatly shortened the progress of a labour that threatened to be long and tedious. It is, indeed, often curious to observe how rapidly the pains increase in force and duration, under the stimulation of gentle and steady pressure at the commencement of each pain. The following case may be taken as a good example of the beneficial effect of pressure applied in this way.

"—, the mother of several children, about thirty-five years of age. Labour commenced at noon on the 23rd February, 1868. The pains were at long intervals, feeble, and of short duration. At 3 a.m. on the morning of the 24th the membranes had been ruptured for several hours, and the os was fully dilated. The pains were now more frequent and regular, but they had no effect in causing the head to pass through the brim. It remained partially engaged, but always receded in the intervals between the pains. After waiting for some time, it seemed as if the forceps would be required. Von Ritgen's method was now tried. The patient being laid on her back, and the hands being spread out on the sides and fundus of the uterus, firm downward pressure was made in the axis of the brim at the commencement of each pain. The good effects of this manœuvre was very striking. The first pain was manifestly increased in strength and duration, and the head was felt to advance decidedly as it was pushed down. The contractions now increased greatly in force, and in about six pains the head was expelled. It was in the third position, and the rotation of the occiput forwards was readily made out as it descended. The child was of immense size, and living. The mother made a good and rapid recovery.

"This may be taken as a typical example of the most usual effect of pressure—viz., to stimulate the uterus to increased exertion; and I believe it to be a far more effective and safe agent for this purpose than ergot.

"Secondly, it is sometimes possible to push out, as it were, the foetus in the entire absence of uterine pains. I presume that cases suitable for this must be rare, and that, as a rule, extraction by the forceps is to be preferred. Still the following case may be taken as proving the possibility of occasionally effecting delivery in this way.

"—, aged twenty-five, a lady of great delicacy of constitution, was

pregnant of her third child. She had suffered a good deal during gestation, was immensely distended with liquor amnii, and for some months had been almost entirely confined to her sofa. Her labour commenced on the 10th of August, 1870. During most of the day she had feeble pains and at long intervals. At 10 p.m. the os was only slightly dilated, and the head was felt to be presenting. The pains got somewhat stronger at 3 a.m., and at 4 a.m. the membranes ruptured, an enormous quantity of water being discharged. At 6 a.m. the os was fully dilated, and the head was engaged in the brim in the first position. The pains were now scarcely worthy of the name. At short intervals there was a barely perceptible hardening of the uterus, which disappeared almost as soon as it was felt, and had no appreciable effect on the presenting part. I was informed that ergot had been administered with advantage in a former labour, and I gave her a full dose without any good result. After waiting till 11 a.m., I began to despair of any progress. The slight contractions previously felt had disappeared, or nearly so, and I made up my mind to apply the forceps.

"The husband, however, objected so strongly to any instrumental interference that I determined to try the effect of pressure, although, in the absence of uterine contractions, I scarcely expected any beneficial results.

"Spreading the hand over the uterus in the usual way, I made firm downward pressure at intervals of from five to ten minutes. The effect was more favourable than I had anticipated. With each application of the pressure the head was felt to descend, and in about three quarters of an hour it was distending the perineum. Now for the first time some slight contraction was felt, and the head was soon expelled. The child was born alive, and the mother made an excellent recovery.

"A case of this sort is no doubt quite exceptional, and I should generally prefer under such circumstances to apply the forceps. Still it may serve to illustrate Kristeller's statement that external pressure alone is capable of effecting delivery. It is, however, as an adjuvant in cases of lingering labour, and as a means of stimulating a feebly contracting uterus, that pressure promises to be of service. I need hardly add, by way of caution, that gentle but firm pressure in a proper direction is to be used, and that all rough handling of the uterus is to be avoided. The pressure can be most readily applied with the patient lying on her back, but this is by no means essential, and I have constantly used it in the ordinary position on the side, and without disturbing the patient."

REPORT ON PUBLIC HEALTH.^a

By CHARLES A. CAMERON, Ph.D., M.D., L.K. & Q.C.P.;
 Prof. of Hygiene in the Royal College of Surgeons; Lecturer
 on Chemistry in Steevens' Hospital Medical College, and the
 Ledwich School of Medicine; Analyst to the City of Dublin.

RELAPSING FEVER.

DOCTOR GEORGE ROSS, Medical Officer of Health for St. Giles' District, London, has published a report on the late epidemic of relapsing fever in that densely populated part of the metropolis. The cases of the disease numbered 241, of which an excessive proportion—97—occurred in the registered, or common lodging houses. Of these dwellings Dr. Ross does not speak favourably. Most of them are kept tolerably clean, and are whitewashed periodically; but they are generally very badly ventilated, have narrow dark staircases, and low ceilings. They are greatly over-crowded. Their kitchens are used as sitting rooms, and on cold or wet days these apartments swarm with lodgers, who render the atmosphere "close, fetid, and noxious." The influence of the insanitary conditions of the common lodging houses in spreading the disease is strikingly shown by the following figures:—

TABLE showing comparative Sick-Rate in the Three Sub-Districts (of St. Giles) and in Common Lodging Houses.

Sub-districts	Population	Cases	Ratios
St. George, Bloomsbury, . . .	17,392	7	1 in 2,484
St. Giles, South	17,940	72	1 in 249
St. Giles, North	16,578	44	1 in 377
Common Lodging Houses . .	2,177	97	1 in 22·4

The facts discovered by Dr. Ross and the results of the inquiries of other investigators show that relapsing fever prevails

^a The author of this report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

exclusively amongst the very lowest classes of the population. As a general rule, privation is a precursor and an accompaniment of the disease, which, indeed, has been termed in Germany and elsewhere, "famine fever."^a Mere poverty does not appear to induce the disease, seeing that costermongers and labourers escape. Absolute want, or actual dearth of nutriment, is, with rare exceptions, the immediate precursor of each attack. When the relapsing fever was established in a locality, the effect of overcrowding in human habitations in spreading the disease became very evident. When it visited a numerous family, it rarely happened that fewer than two members suffered. Very frequently all the children in a family were attacked.

Dr. Ross ascertained that there were no apparent relations between the spread of relapsing fever and the temperature or humidity of the atmosphere. As cold lowers the vital powers of persons insufficiently fed, we should have supposed *à priori* that a low temperature would have increased the amount of the disease.

Dr. Ross believes that the virus of relapsing fever is a distinct entity, and that it is probably of vegetable origin. He considers that it is a weak poison, and easily destroyed; hence its victims are generally half starved or weakly individuals, whose feeble organizations are unable to resist the least powerful of the morbid agents. That the poison of the disease was floating about in the air appears very probable from the fact that the fever increased inversely as the atmospheric ozone decreased. When the mean amount of ozone was so low as 0·4 at Greenwich, the cases of the disease amounted to 3·4 each day; when the ozone rose to 1, the attacks decreased to 3 per diem. The days on which the ozone stood at 2, the attacks were 1; at 3, the cases of disease were 0·9 daily; and when the ozone increased to 5, the attacks sank to 0·3 daily. These facts certainly do not appear to be mere coincidences. They show a close relation between the amount of atmospheric ozone and the intensity of a zymotic disease. They do not, of course, prove—that which has been so often alleged—that deficiency of ozone in the air is the direct cause of disease; but they certainly go far to prove that ozone is capable of oxidizing the morbid agents which float in the air. It is to be regretted that Dr. Ross did not determine the amount of ozone in the atmosphere of his district. St. Giles is some distance from the Observatory at Greenwich. At the same time it must be

^a Virchow, of Berlin, describes it as "*die hunger-pest.*"

admitted that the amount of ozone in the air at Greenwich would be about the quantity which should exist in the atmosphere of all parts of the metropolis, provided it were not used up in oxidizing or ozonizing the organic matters and sulphuretted hydrogen gas, which occur in sensible quantities in "London smoke." It would be desirable that every medical officer of health should provide himself with the most modern and useful form of ozonimeter. The mode of using such an instrument is very simple; and if it were constantly employed we have no doubt but that valuable medical results would follow.

The rate of mortality from relapsing fever in Dr. Ross's district was only two and a half per cent., whereas in some other places it amounted to about ten per cent. This small mortality appears to have been due to the prompt sanitary measures employed, but more especially to the instant removal of the sick to the hospitals, where they received not only medicine but good food. We believe that fevers of all kinds might be greatly lessened were the affected at once removed to hospital, and everything which had been in contact with their bodies or breath cleansed and disinfected. The patients should also be allowed to remain in hospital or in a convalescent institution until it became evident that he was no longer likely to be a source of contagion. At the present time great efforts are being made in Dublin to secure the prompt removal of fever patients to hospital. Every morning a sanitary sergeant of police calls upon each dispensary physician, and ascertains from him the name and address of those of his patients who are affected with any zymotic disease. This information is acted upon at once. The patient is, if at all possible, immediately conveyed to the hospital, and the apartment in which he had lived is disinfected. The great difficulty is to disinfect the clothes of the persons who live with the sick. Amongst the poorer classes a change of raiment is the exception not the rule. To provide them with new clothes is somewhat expensive, and the public health authorities are not likely to be induced to sanction any large expenditure for such a purpose. Nevertheless, it is probable, that what might appear to be the most expensive method of endeavouring to "stamp out" or greatly lessen zymotic disease, would in the end be the cheapest plan. The rinderpest was extirpated in England and Scotland by the most expensive method that could possibly be adopted—namely, the instant destruction of every animal that exhibited the first symptoms of the disease. Thousands of animals were killed, but with them also perished the disease,

which otherwise might have lingered for years in this country (as was the case in the last century), carrying off annually a large number of animals. The contagious pleuro-pneumonia, or lung distemper of oxen, which now may be regarded as endemic in these countries, might have been extirpated the first year it was imported from the continent. The attempt is now about to be tried; but the operations of the Contagious Diseases (Animal) Prevention Act^a will be found of far more difficult application now, than they would have been in the year 1841, when we first became acquainted with this epizootic.

It would be most desirable to make an energetic and sustained attempt to root out one of the zymotics, for were such an effort to prove successful, it would encourage the public health authorities to expend considerable sums in the execution of sanitary measures. A rich corporation—for example that of London, whose revenue in relation to the population of their district is enormous—should undertake some such experiment.

Of all zymotics, scarlatina is probably the most difficult to be controlled by sanitary measures. Whatever may be the nature of its virus, or contagium, it certainly possesses a wonderful power of resisting ordinary atmospheric influences. That the poison of scarlatina may remain intact for many months in clothes and wall paper is a fact too often rendered fatally evident. Convalescents after an attack of this disease appear to remain for a longer period capable of infecting healthy persons, than convalescents from other zymotics. We would not, therefore, propose to begin with scarlatina, but with measles or whooping cough, or some other zymotic with a less persistent poison than that of scarlatina. The speedy isolation of every person suffering from measles, the thorough disinfection of everything which had been in contact with them, and the retention of the patients in hospital until the restoration of their health and strength guaranteed that the poison of the disease had been eliminated from their systems; these are the measures which, if persistently carried out for a few months, would, we have a strong belief, eradicate the disease. It is evident that the present system of sanitary inspection and police hygiene in towns, can do no more at the outside than

^a This Act was passed late in the last Session of Parliament. It prohibits the removal of any animal suffering from a contagious disease, unless by virtue of a permit from the proper authorities. The maximum penalty for violating the provisions of this Act, is three months' imprisonment with hard labour. As it is likely to lessen the traffic in diseased meat, this Act may be regarded as an excellent piece of sanitary legislation.

to lessen the amount of zymotic disease. It is only a *severe* and even apparently a tyrannical method of dealing with this serious matter that could possibly annihilate the contagious maladies. The labour is, no doubt, herculean, but the difficulties to be overcome are not unsurmountable; and when it is considered that success would mean the saving of a fourth of the lives that annually perish in these countries, surely almost no sacrifice could be too great to accomplish such an object!

The following graphic description of relapsing fever is given by Dr. J. B. Russell, physician superintendent of the Glasgow Fever Hospital, in his report of that institution, for the year ended on the 30th April, 1870:—

“The distinctive peculiarity of relapsing fever is of course the *relapse*. The patient is suddenly seized with a violent attack of fever, generally accompanied with recurring perspiration. This passes off with remarkable suddenness, and usually with profuse sweating, about the seventh day. The patient is then left pale, emaciated, and exhausted in appearance—to use a vulgar but expressive phrase, “washed out.” The appetite speedily returns, the patient rapidly regains strength, feels perfectly well, and is able, if permitted, to walk about with ease. But about the fifteenth day from the first access of fever, the relapse occurs: all the previous symptoms return, and the fever prevails as before for another period of six or seven days, passing off in the same sudden manner, usually with profuse perspirations. The patient is left still more blanched and exhausted; but the appetite soon becomes ravenous, and the repair of the bodily waste is rapid in young people, but tedious in the aged. Though not a fatal, relapsing fever is a very painful and distressing disease. It is very generally accompanied by severe pains in the limbs, and often in various parts of the body. The patient is throughout the febrile periods manifestly disturbed and miserable, while at the crisis the temporary prostration is excessive, the voice becomes quite puerile, and there is a disagreeable sense of sinking. I believe this obtrusive appearance of discomfort arises partly from the fact that the disease does not invade the brain and benumb the senses; so that whatever painful disturbance of the system it produces is fully felt and displayed. Consequently, to an inexperienced eye, a relapsing fever patient, as contrasted with one suffering from typhus, seems much the worse of the two. The former is brightly flushed, moaning, restless, breathing in a panting manner, and querulous; while the latter is dingy in countenance, dull and stolid in expression, deaf and stupid, and, if asked how he feels, generally replies, “very well.” As already stated, the mean feature of famine fever, as distinguished from typhus, is the relapse after a non-febrile interval,

But for this, the fever runs so high that, if prolonged unbroken, as in typhus for twelve or fourteen days, it would almost certainly prove fatal. A rash occurs in many cases, which is often scarcely distinguishable from that of typhus; but, unlike the rash of typhus, it is not always present, nor constant in the time of its appearance, is transitory, and never assumes the purple or black hue of the typhus eruption. As relapsing fever is specifically different from typhus, the one does not afford protection from the other. Indeed, I should expect that typhus would spread with unusual rapidity and ease among a poor community after being emaciated and broken in health by relapsing fever. The two are, however, congeners, evidently related in their pedigree."

Dr. Russell gives in this Report a number of interesting facts illustrative of the propagation of typhus, from which he concludes:—

"(1) That where attention is paid to personal and general cleanliness typhus does not carry far, so to speak, through the atmosphere, and is not portable; (2) close approach to, and contact with, the infected individual and his dirty belongings lead with great certainty, even in the best sanitary circumstances, and in healthy and well-fed people, to an attack at the end of about four weeks in the majority of cases, but not in a few until the lapse even of some months; (3) that individual insusceptibility does not exist, except that which is conferred by a previous attack. As an interesting contrast with our experience of typhus, I may say that no case of enteric fever has ever arisen either among the staff or among the patients beside whom cases of enteric fever are treated. These latter have, however, in a very few cases caught typhus."

VACCINATION.

Mr. George S. Gibbs has "translated and abridged" a report, on vaccination in France, which, in its English dress, at least, appears to show that vaccination causes, not a diminution of small-pox, but an increase in the amount and fatality of the disease. The following is one of the tables contained in this pamphlet:—

* Report presented to His Excellency the Minister for Agriculture, Commerce, and Public Works, by the Imperial Academy of Medicine respecting the Vaccinations performed in France, during the year 1867. "Paris, Imprimerie Imperiale, MDCCCLXX." Translated and Abridged, with the Arithmetical Proportions of the Statistics, calculated and arranged by George S. Gibbs. Also, an Appendix of Statistics relating to the years 1865 and 1866. London: Longmans, Green, Reader, and Dyer. 1870.

TABLE III.—1867.

Number of Departments in France	Per centage of Vaccinations and Re-Vaccinations to Births	Per centage of Smallpox cases to Births	Per centage of Smallpox Deaths to Births	Per centage of Smallpox Deaths to Smallpox Cases
SEVENTY-SEVEN,—all for which the Reports are complete,—extremes }	16 to 133	0·0 to 15·14	0·0 to 1·69	0·0 to 2·5
Average - - -	65	2·08	0·22	10·1
TEN,—in which Vaccination is <i>least</i> practised, —extremes - - - }	16 to 26	0·0 to 4·4	0·00 to 0·42	0·0 to 21·2
Average - - -	24	0·88	0·01	9·7
TEN,—in which Vaccination is <i>most</i> practised, —extremes - - - }	93 to 133	0·0 to 14·2	0·0 to 1·59	0·0 to 20·0
Average - - -	105	4·27	0·49	11·6
23 in which the proportion of Vaccinations to Births does not exceed 50 per cent.,—extremes }	16 to 50	0·0 to 4·4	0·00 to 0·42	0·00 to 37·5
Average - - -	34	0·83	0·08	9·2
54 in which the proportion of Vaccinations to Births is more than 50 per cent.,—extremes - }	51 to 133	0·0 to 15·14	0·10 to 1·69	0·0 to 72·5
Average - - -	77	2·54	0·28	10·6

SUMMARY OF AVERAGES.

	Ten Departments		Departments	
	Least Vaccinated	Most Vaccinated	23 Least Vaccinated	54 Most Vaccinated
Smallpox cases to Births	0·88	4·27	0·83	2·54
Smallpox Deaths to Births	0·01	0·49	0·08	0·28
Smallpox Deaths to Cases	9·7	11·6	9·2	10·6

From these and other tables of statistics Mr. Gibbs deduces certain conclusions, or “readings,” which, if warranted by facts, would

certainly prove that, in France at least, vaccination was an evil rather than a blessing. We shall only quote part of Mr. Gibb's readings:—

“In the summary of averages, the figures for the two sets of ten departments and for the two portions of the whole country are brought into juxtaposition to facilitate comparison. The reading is:—for every eighty-eight cases of smallpox occurring in the ten departments least vaccinated, there occurred 427 in the ten most vaccinated; for every eighty-three cases occurring in the twenty-three departments least vaccinated, there occurred 254 in the fifty-four most vaccinated. Smallpox deaths:—for every one occurring in the ten departments least vaccinated, there occurred forty-nine in the ten most vaccinated; for every eight occurring in the twenty-three departments least vaccinated there occurred twenty-eight in the fifty-four most vaccinated. Proportion of smallpox deaths to smallpox cases:—out of every thousand cases occurring in the ten departments least vaccinated there died 97; out of every thousand cases occurring in the ten most vaccinated there died 116; out of every thousand cases occurring in the twenty-three departments least vaccinated there died 92; out of every thousand cases occurring in the fifty-four departments most vaccinated there died 106.”

It appears to us that a careful study of the statistics given in Mr. Gibb's pamphlet, would show that they do not really disprove the prophylactic virtues of vaccination. In the first place, we notice that no information is given relative to the former sanitary condition of the departments where vaccination is most or least practised. It would have been important to show whether or not smallpox had increased or decreased within say the last ten years in both classes of departments. It might, for example, be found that in a department where in 1867, there were high proportions of vaccinations and smallpox cases to births, there were in 1857 a lower proportion of vaccinations and a higher ratio of smallpox attacks.

In order to establish the proposition that the number of smallpox cases stood in a direct relation to the number of vaccinations, it should be shown that such a condition of things prevailed generally throughout all the departments of France. We find, however, that in several of the “most vaccinated” departments there were few or no cases of smallpox; while, on the other hand, in several of the “least vaccinated” departments there were large proportions of smallpox attacks to births. In order to show how fallacious it would be to deduce from the statistics in this pamphlet any conclusions

adverse to the practice of vaccination, we submit the following "readings" of Mr. Gibb's figures.

In the ten departments where Mr. Gibbs says there were the fewest vaccinations and the fewest cases of smallpox the following figures apply:—

Departments		Per centage of Vac- cinations and Re-Vaccinations to Births	Per centage of Smallpox cases to Births	Per centage of Small- pox Deaths to Births
Creuze	-	16	0.10	0.00
Corse	-	17	0.91	0.18
Ardennes	-	20	0.00	0.00
Aveyron	-	19	0.35	0.02
Cher.	-	21	0.00	0.00
Alpes, Maritimes	-	25	1.97	0.42
Loiret	-	25	0.37	0.02
Ariège	-	28	0.93	0.14
Hérault	-	28	1.41	0.18
Manche	-	28	0.00	0.00
Average	-	22.7	0.404	0.096

The total number of births amounted only to 77,856.

We shall now compare with these figures the statistics of ten of the departments where vaccinations were most practised:—

Departments		Per centage of Vac- cinations and Re-Vaccinations to Births	Per centage of Smallpox cases to Births.	Per centage of small- pox Deaths Births
Charente Inferieure		93	0.52	0.05
Pas de Calais	-	82	0.23	0.00
Vosges	-	84	0.27	0.05
Vendée	-	96	0.00	0.00
Corrièze	-	110	0.06	0.01
Seine et Oise	-	95	0.28	0.02
Lot et Garonne	-	94	0.29	0.05
Pyrenees, Basses	-	85	0.00	0.00
Aube	-	85	0.29	0.02
Seine et Marne	-	103	0.08	0.00
Average	-	92.7	0.202	0.015

The total number of births amounted to 94,395.

The large number of smallpox cases in the ten departments which

Mr. Gibbs describes as "most vaccinated" include six in which there were few or no cases. The prevalence of smallpox in the ten departments was, in fact, owing to epidemics of the disease which occurred in four of them. No information is afforded relative to the immunity from smallpox of the vaccinated, as compared with the unvaccinated. On the whole we are satisfied that there is nothing in Mr. Gibbs' pamphlet which should lead us to modify the opinion generally entertained relative to the value of vaccination as a prophylactic agent.

It is well known that in some parts of the continent of Europe it is the practice to vaccinate the human subject by means of lymph taken direct from the ox. The supply of contagium is kept up by inoculating a succession of calves. The statement so persistently made that syphilis is often introduced into the system of infants by means of the vaccine matter would, if true, be a strong reason for the general adoption of what is termed "Animal Vaccination."

In the annual report for 1869, the medical officer of the Privy Council, Dr. Simon, deals in an exhaustive manner with this question, and shows that when vaccination is properly performed, it could not by possibility be the means of introducing syphilis into the system. He examines into all the alleged cases of syphilitic inoculation by means of impure vaccine lymph, and reduces them to 14 in number, in which "the so-called vaccinator did some how or other produce the result which was attributed to him." All these cases Dr. Simon believes to have been the result of *mala praxis*. The operator may by some slovenly or careless act have mixed the poison of syphilis with the vaccine matter; or he may have altogether substituted the former for the latter. "If," says Dr. Simon, "our ordinary current vaccination propagates syphilis, where is the syphilis that it propagates? Who sees it? The experience of this department is an entire blank upon the subject. For the last ten years we have been in incessant intimate communication with the different parts of England on details of public vaccination, and during these ten years every one of about 2,500 vaccination districts, into which England is divided, has been visited three or four times by an inspector, specially charged with the duty of minutely investigating the local practice of vaccination; yet from this systematic and extremely detailed search for all that has to be said on the subject of vaccination in England, no inspector has ever reported any local accusation or suspicion that a vaccinator had communicated syphilis."

Schreier of Ratisbon, Heim of Wurtemberg, Boeck of Christiana (so well known for his investigations relative to the inoculability of syphilis), and Taupin of Paris, have designedly employed vaccinated persons with lymph taken from syphilitic children, yet in no one instance did they succeed in communicating the syphilitic taint by that method. In Boeck's experiments care was taken to mingle blood with the lymph so that it might not be said that although lymph had failed to carry the poison of syphilis, the blood of the child might have contained the contagion.

In his report of 1869, Dr. Simon gives the results of Dr. Seator's inquiries relative to animal vaccination on the continent. They show that it would be most undesirable to adopt that system in these countries. It would appear that even the most careful practitioners find it difficult, and sometimes impossible, to transmit "successive vaccination from calf to calf without very frequent recurrence of failures and interruptions." The proportion of unsuccessful attempts to vaccinate perfectly, to successful efforts, appears to be exceedingly high. At Rotterdam, for example, the proportion of unsuccess was nearly twenty times greater than in the case of arm-to-arm vaccination. With respect to the preservability of lymph, we learn that specimens of human and animal lymph had been sent from Holland to one of the eastern possessions of that kingdom, undergoing in the transit considerable alternations of temperature. Of forty-four children who had been vaccinated with the human lymph, sixteen exhibited all the symptoms of vaccinia; whilst in not one of forty-seven cases in which the animal lymph had been employed, had successful vaccination been the result.

THE SEWAGE NUISANCE.

The evils which arose from the practice of storing the waste matters of towns in cesspools, were, to a very great extent, remedied by the general introduction some thirty years ago of the sewerage system. Of late years, however, the sewerage system has itself become a great nuisance, and an extensive source of disease. There is no doubt but that a most effectual, expeditious, and cleanly way of removing the ejesta of the population is by means of water. A good and well kept water-closet, connected with a properly constructed sewer, ought to dispose of excremental matter in a satisfactory manner. In many towns, however, it unfortunately happens that the excremental matters are conveyed only a short distance in order to be deposited in rivers, which are thereby

converted into huge open sewers. The pollution of our water courses has now, in fact, become so great an evil owing to the extensive development of the town sewerage system, that many sanitarians maintain that the best remedy would be a total discontinuance of the use of water-closets. In the lately issued Annual Report of the Medical Officers of the Privy Council, we find two most voluminous reports upon this subject, one by Dr. A. Buchanan, the other the joint production of Dr. Buchanan and Mr. J. Netten Radcliffe. Dr. Buchanan writes strongly in favour of the dry-earth system of dealing with excrements, and concludes his elaborate report by stating that his inquiries lead to the following conclusions:—

“(1.) The earth closet, intelligently managed, furnishes a means of disposing of excrement without nuisance, and apparently without detriment to health.

“(2.) In communities, the earth closet system requires to be managed by the authority of the place, and will pay at least the expenses of its management.

“(3.) In the poorer class of houses, where supervision of any closet arrangements is indispensable, the adoption of the earth system offers especial advantages.

“(4.) The earth system of excrement removal does not supersede the necessity for an independent means of removing slops, rain water, and soil water.

“(5.) The limits of application of the earth system in the future cannot be stated. In existing towns, favourably arranged for access to the closets, the system might at once be applied to populations of 10,000 persons.

“(6.) As compared with the water-closet, the earth-closet has these advantages:—It is cheaper in original cost; it requires less repair; it is not injured by frost; it is not damaged by improper substances being thrown down it, and it very greatly reduces the quantity of water required by each household.

“(7.) As regards the application of excrement to the land, the advantages of the earth system are these: the whole agricultural value of the excrement is retained; the resulting manure is in a state in which it can be kept, carried about, and applied to crops with facility; there is no need for restricting its use to any particular area, nor for using it at times when agriculturally it is worthless, and it can be applied with advantage to a very great variety, if not to all, crops and soils. After the disposal of excrement by earth, irrigation will continue to have its value as a means of extracting from the refuse water of a place whatever agricultural value it may possess for the benefit of such crops and such places as can advantageously be subjected to the process.

“(8.) These conclusions have no reference to the disposal of trade or manufacturing refuse, which it is assumed ought to be dealt with, as belonging to the business in which it is produced, by the people who produce it, and not to come within the province of local authorities to provide for.”

The report by Dr. Buchanan and Mr. Radcliffe describes the actual method of getting rid of excremental matters in various villages and towns in England and Scotland, and is illustrated with numerous engravings of water-closets, earth-closets, privies, middens, ash-pits, sewage-tanks, and carts, vans, &c., for the removal of night soil and other refuse. The general conclusions at which they arrive are as follows:—

“1. Excrement may be removed from a town and safely disposed of on more than one principle, and the same principle does not need to be applied in all quarters of the same town.

“2. As regards the parts of a town inhabited by the poorer classes, a water-closet system may be managed so as to be entirely applicable to the circumstances of the most ignorant and most careless population. Essential conditions of such applicability, however, are that the structural arrangements should be adapted to their purpose and be independent of the person using the closet, and that the management should be wholly undertaken and efficiently done by the servants of the sanitary authority. Where these conditions are observed as thoroughly as they are observed in parts of Liverpool, we believe that water-closets are the best means of removing excremental matters from the poor neighbourhoods of a town.

“3. The earth system affords a second way of safely disposing of excrement. It is, as shown in the special report on earth-closets, an essential element in this system also, as applied in poor neighbourhoods, that the entire management of it shall be conducted by the sanitary authority.

“4. The midden system may be modified so as greatly to reduce nuisance and danger from it. We have described the form of midden-closet which we think presents fewest objections. We cannot speak of satisfactory safety in the use of even this form of midden-closet, partly because we hardly expect to see it carried out with daily emptying, and partly because the materials of the midden would probably be retentive of some excremental matters; but if, under certain circumstances, middens constructed as above should be tolerated, it would, we think, be scarcely less than essential, first, that they should, if in a densely populated neighbourhood, be emptied daily, or, under other circumstances, at least once a week; and secondly, that the arrangements for excrement

removal should be wholly in the hands of efficient persons appointed by the sanitary authority.

"5. The pail system presents several advantages for poor town districts. It may safely be employed for excrement-removal if movable pails of defined construction be used, and be changed every day for fresh pails. Such a system, involving similar construction or constructive alterations as are required for the toleration of a middle system, offers advantages over the latter in regard of facility for frequent removal of excrement, in regard of safety from nuisance, and probably in regard of profit in disposing of excrement as manure.

"6. Those who use the closet may, both under the pail and the midden system, be expected, with due superintendence, to do the cleansing of it so far as merely affects ordinary comfort and decency; but such action as concerns the effectiveness of the closet as a means of excrement-removal must be taken by the sanitary authority itself.

"7. If these conclusions be accepted, it follows that there are various demonstrable methods which will fairly answer the purpose of preventing nuisance and injury to health from the retention of excrement, at least until the perfection of arrangements for dealing with excrement shall be agreed upon. It cannot yet be affirmed of any one of the methods that it will develop into the only perfect system of the future."

We learn from the Report of the New York Board of Health that in the metropolitan district the persons employed in removing night soil must be duly licensed; and they are obliged to disinfect the privy stuff before removing it to their carts. We are not aware that a similar practice prevails in any town in the United Kingdom.

Should the earth closet supersede the water closet in large towns we think it should be of sufficient dimensions to retain all the liquid as well as the solid ejesta produced in each household. As a manure the liquid excrement of the population is about twenty times more valuable than the solid. There is now a very general disposition on the part of Municipal authorities to attempt the utilization of town sewage. The Corporation of Dublin have just resolved to spend a quarter of a million on the construction of sewerage works, by which the city drainage will not be merely kept out of the Liffey, but will be applied to the purpose of fertilizing the soil. To render the dry closet plan as great a pecuniary success as possible, it should deal perfectly with the liquid excrement of the population, which, as we have seen, is, as a fertilizer, the really valuable part of excremental matter. Medical Officers of Health and Army Surgeons would not fail to derive

valuable practical information from the reports of Dr. Buchanan and Mr. Radcliffe.

In connexion with this subject we may here mention that Dr. Corfield has just published a large work^a in which all the more important facts in relation to the treatment and utilization of sewage are given. He prepared it at the request of a Committee of the British Association appointed last year to collect and arrange what might be termed the facts of the sewage question. Dr. Corfield has carefully executed the task entrusted to him; but his work is not a mere compilation, as it contains original critical opinions, suggestions, and observations, which materially enrich its pages. It is a valuable contribution to the literature of the sewage question.

HEALTH OF THE NAVY.

We have just received, through the kindness of Dr. Armstrong, Director-General of the Medical Department of the Navy, the "Statistical Report of the Health of the Navy for 1867," printed by order of the House of Commons. It is a large volume, of nearly 600 pages, and is illustrated by several maps and numerous charts, or diagrams. We hope that a copy of it is supplied to every Naval Medical Officer. The Report states that, as compared with the average of the ten preceding years, there was a decided decrease of sickness, invaliding, and mortality. The total death-rate from disease alone was in the annual ratio of 8·4 per 1,000 living, and the deaths from violence 3 per 1,000. On the Home station the deaths were 5·8 per 1,000 from disease, and 1·6 per 1,000 from violence. On the West Coast of Africa the mortality from disease was 22·8 per 1,000; in China, 18·2; Australia, 9·4; Pacific, 5·2; and North America and the West Indies 8·6 per 1,000 from disease alone. The recent sanitary statistics of the navy prove that the hygiene of ships has been carefully attended to in recent years, and that our Men-of-War are now in a condition very different to that described by Smollett in the humorous but truthful pages of *Roderick Random*.

The most interesting—to general readers at least—of the appendices to the Report is a paper on the Dieting of Seamen, contributed by Alexander Rattray, Surgeon R.N. The writer, whilst acknowledging the great change for the better effected of late

^a A Digest of Facts relating to the Treatment and Utilization of Sewage. By W. H. Corfield, M.A., M.B., Professor of Hygiene and Public Health, University College, London. London: Macmillan and Co. 1870. 232 pages.

years in the sanitary condition of seamen, maintains that there is still great room for improvement relative to their food. He points out the defects of the present dietaries of seamen—which appear to be nearly the same under all climatic conditions—and suggests the following dietaries:—

Proposed Naval Dietary.

		Temperate, where the Daily or Annual Range of Temperature is below 70 Fahr.	Tropical, where the Daily or Annual Range of Temperature is above 70 Fahr.
		Biscuit, or soft bread (r) - - - - - 1½ lb.	1½ lb.
		Sugar - - - - - (a) 2⅓ oz.	(b) 2 oz.
		Chocolate (c) - - - - - 1 oz.	1 oz.
		Tea - - - - - ¼ oz.	¼ oz.
		Coffee - - - - - (d) ⅓ oz.	—
		Lime-juice - - - - - (e) ½ oz.	(f) ½ oz.
		Sugar for ditto - - - - - (e) ½ oz.	(f) ½ oz.
In Harbour (when procurable).			
Daily -	Fresh meat (g) - - - - -	1 lb.	12 oz.
	Vegetables (h) - - - - -	(h) ½ lb.	12 oz.
	Or Rice - - - - -	(k) 6 oz.	9 oz.
	Molasses - - - - -	(k) 1 oz.	1½ oz.
		At Sea (or when Fresh Provisions cannot be procured).	
	Preserved meat (l) - - - - -	¾ lb.	(o) 8 oz.
	Pearl barley (s) - - - - -	2½ oz.	3 oz.
	Or Flour - - - - -	9 oz.	12 oz.
	Suet - - - - -	¾ oz.	¾ oz.
	Raisins - - - - -	1½ oz.	2 oz.
	Or split peas for soup (p) - - - - -	⅓ lb.	½ lb.
	Or Rice - - - - -	(m) 6 oz.	9 oz.
	Molasses - - - - -	(m) 1 oz.	1½ oz.
In Harbour and at Sea.			
Once a Week -	Salt meat (n) - - - - -	1 lb.	12 oz.
Weekly -	Mustard - - - - -	½ oz.	½ oz.
	Pepper - - - - -	¼ oz.	¼ oz.
	Vinegar - - - - -	½ gill	½ gill
	Oatmeal - - - - -	3 oz.	3 oz.
	Celery seed - - - - -	½ oz. to 8 lbs. peas, or 24 men.	

(a) One oz. each for the chocolate and tea, $\frac{1}{2}$ oz. for the coffee.—(b) For equal division among the two following.—(c) At sea, in addition, $\frac{1}{2}$ oz. chocolate and $\frac{1}{2}$ oz. sugar per man of the middle and morning watches.—(d) After dinner, in lieu of the spirit ration.—(e) After ten days at sea, on the surgeon's recommendation.—(f) In lieu of the grog ration.—(g) Beef and mutton alternately, and occasionally pork in cold climates; for variety, these might be sometimes roasted.—(h) Succulent, alternately with potatoes; in the tropics, yams and pumpkins; for variety, the potatoes and rice might now and then be converted into soup.—(i) During Summer.—(l) Beef and mutton alternately.—(m) During summer, and for the semi-tropical dietary.—(n) Beef

and pork alternately ; in harbour, the former with pudding, and the pork with pea-soup rations ; at sea, the beef on pudding, and pork on pea-soup, days.—(o) Cold, if preferred.—(p) When pea-soup falls on preserved meat days, the 4, 6, or 8 lb. piece of salt pork or beef (necessary ?) for making it may be deducted from the subsequent issue of salt meat.—(r) At discretion in harbour.—(s) For broth, with the preserved meat.

DOCTRINE OF CONTAGION.

Tyndall's remarkable lecture on atmospheric dust gave a degree of popularity to that subject which all the labours of previous investigators had failed to secure for it. Huxley's eloquent and learned address, at the late meeting of the British Medical Association, has in a similar manner directed the attention of the reading public to what we may term the doctrine of contagion. The whole address is worthy of a careful study, as it is replete with valuable suggestions ; but it is in relation to the subject of the germs of disease that we wish particularly to direct attention to it. Professor Huxley believes that the poison of the zymotics is composed of small living bodies, which may be termed *microzymes*. Speaking of vaccination, he says :—

“A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter by its irritative property produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active elements in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding 1·20,000th of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are almost dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by ‘infection’ or ‘contagion,’ to fresh wort. In both cases it is the solid living particles which are efficient ; the liquid in which they float, and at the expense of which they live, being altogether passive.

"Now arises the question, are these microzymes the results of *Homo-genesis*, or of *Xenogenesis*; are they capable, like the *Torulæ* of yeast, of arising only by the development of pre-existing germs; or may they be, like the constituents of a nut-gall, the result of a modification and individualization of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called 'heterologous growths?' It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by destroying its germ, but a pathological product can only be annihilated by removing the conditions which give rise to it.

"It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favour of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favour of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

"It is, at present, a well-established fact, that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape disease and the potato disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*."

After describing various diseases in the lower animals, which are undoubtedly produced by the presence of the very lowest forms of living things, Professor Huxley concludes as follows:—

"It is now certain that this devastating, cholera-like, Pébrine, is the effect of the growth and multiplication of the *Panhistophyton* in the silkworm. It is contagious and infectious because the corpuscles of the *panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighbourhood; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid; and for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine but has received its explanation from the fact that the disease is the result of the presence of microscopic organism, *Panhistophyton*.

"Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this

depends upon the way in which the *Pankhistophyton* is generated. If it may be generated by Abiogenesis, or by Zenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Pankhistophyton* is an independent organism, which is no more generated by the silkworm than the misletoe is generated by the oak, or the apple-tree, on which it grows, though it may need the silkworm for its development in the same way as the misletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of, and keep away the germs of the *pankhistophyton*. As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and guided by that theory, he has devised a method of extirpating the disease, which has proved to be completely successful wherever it has been properly carried out.

"There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from pre-existing germs, or by Homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for this evidence to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It seems to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skilful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet, or bayonet, are due to the importation of minute organisms into the wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi."

In the current volume of the Annual Report of the Medical Officer of the Privy Council we find an "Introductory Report on the Intimate Pathology of Contagion," by Dr. Burdon Sanderson. The reporter gives a clear description of the labours of the mycologists—chiefly the German—who have been endeavouring to prove that zymotics and epizootics are produced by low forms of vegetable life. Dr. Sanderson believes that these researches have established at least one fundamental doctrine—namely, that every kind of contagium consists of particles. The minutest organisms

yet discovered are the spheroidal corpuscles termed *micrococcus*, by Hallier, and *microzymæ* by Béchamp. These minute bodies are probably the poison of all kind of contagious diseases. Their minuteness renders very difficult their accurate examination; but there is no doubt that they do not all possess the same specific properties. All *microzymæ* may not be *contagium*, but all kinds of *contagium* are in all probability *microzymæ*. This subject is one of great interest, and is admirably treated in Dr. Sanderson's report. In the volume containing Dr. Sanderson's report there is one by Thudicum on "Researches, intended to promote an improved chemical identification of disease." It is one of a series of reports which have been or are to be published, and when concluded they will undoubtedly afford most valuable results to pathology. At present we propose merely to refer to these important and laborious investigations.

ARE ZYMOTIC DISEASES PREVENTABLE?

Dr. Starks, Registrar-General of Vital Statistics in Scotland, expresses his belief on the impossibility of extirpating the zymotic disease by sanitary means. He states^a that in the healthy rural districts there is about the same relation between the number of deaths from contagious and non-contagious as that which exists in towns.

The sanitary conditions of the villages, and even single houses in rural districts, are often in every way very unsatisfactory, and are favourable to the spread of zymotics; but, on the whole, the mortality in the country is much less than in towns. It must, however, be borne in mind that the general adoption of such sanitary measures as sewerage, ventilation, cleanliness, good water supplies, &c., re-act favourably upon other diseases as well as the zymotics. For example, the drainage of most of the larger towns in England has caused, as we have shown in a former report, a great diminution in the mortality from phthisis. We have no doubt but that sanitary improvements in towns reduces the death-rate from non-contagious diseases, as well as the mortality from zymotics. In Merthyr-Tydvil, for example, the recent sanitary improvements have caused a far greater relative reduction in the death-rate from the zymotics than from the non-contagious maladies. Sunderland formerly was a most unhealthy town, owing to the prevalence of zymotics, especially fever. The study of the vital statistics of this large town for the last ten years shows that the great increase in its health is to a very large extent due to the

^a Edinburgh Medical Journal, December, 1869, and January, 1870.

diminution of zymotics—the result of sanitary improvements. Here I have to thank Mr. Joshua Wilson, Chairman of the Sewerage and Health Department of the town, for a mass of valuable information, statistical and otherwise, relative to that town. The *Sunderland Times* for June 28, 1870, says:—

“Owing to the perseverance of a handful of enlightened men, of whom many now rest from their labours, thankless so far as the frivolously busy world goes, but worthy of all praise, the town has now an abundant supply of pure water, a system of sewerage as near perfection as may be, a public park which can be shown with pride to strangers, and all the main hygienic appliances to make urban life what it should be. The Local Board of Health can call the attention of the Privy Council to these important facts, and let them know at the same time that the intelligent burgesses of Sunderland are by no means disposed to rest content with what has been done towards making the town the healthiest of its size, on the whole, in the United Kingdom. In spite of all ignorant, short-sighted, or factious opposition, the majority of them are determined to go through with the grand scheme of Town Improvement devised by the late Ald. Williams, subject, of course, to such modifications as experience may suggest, and at such a rate of progress as the state of trade and other things warrant.”

IS DIARRHEA A TRUE ZYMOTIC?

We have often expressed our belief that diarrhea is not a true zymotic, or at least that it should not be placed in the same category of diseases which include small-pox, typhus, and scarlatina. It is not probably that the poison of this malady is a specific entity like the virus of measles or cholera. As bronchitis prevails in very cold weather, for an obvious reason, so diarrhea prevails in summer when fruit is abundant. Dr. J. Wyatt Crane, of Leicester, contends^a that diarrhea is not a true zymotic, and is not, therefore, in the ordinary sense of the word, a preventable disease. Of course, so far as private hygiene is concerned, there are many means of lessening the amount of diarrhea; for example, by paying attention to diet, so as to avoid using foods which are likely to produce diarrhea. Public hygiene may also do something in the direction of lessening the amount of diarrhea, by preventing the sale of unsound food, and taking care that the milk which is so largely used by children is supplied to them in a pure condition. Still, we believe that some distinction should be made between diarrhea and the undoubtedly contagious diseases.

^a Report of the Sanitary Condition of Leicester for 1869, by J. Wyatt Crane, M.D., Medical Officer of Health. Leicester: J. and T. Spencer. 1870.

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

MR. PORTER, President.

Cancer of the Uterus.—DR. LITTLE said although he had no clinical history of this case there were some points about it worth recording. He was indebted for the opportunity of exhibiting the specimen to Dr. Battersby, who had found it in the body of a woman which had been brought into the Ledwich School of Medicine. This diffuent structure, which looked more like brain tissue than anything else, consists of the uterus converted into a mass of encephaloid cancer. Neither the rectum, bladder, vagina, nor os uteri, was invaded by the disease, but the calibre of the rectum was diminished by pressure. In cases where obstetricians suspected malignant disease of the uterus, they were in the habit of introducing a speculum, and looking for appearances at the os—namely, an os patulous and ulcerous; whereas had the speculum been introduced in this case, the os would have been found small, and with no apparent disease. In the cancerous mass there were found two cysts, but he could not say whether they had sprung from the ovary or not. The liver showed cancerous deposits on its surface, but there was no disease in its interior. On the under surface of the diaphragm cancerous nodules of a similar nature were to be seen. The position of the malignant deposit in the liver was interesting. There were apparently two ways in which malignant disease was capable of spreading. One was when the infective material found its way into the veins, was carried to the centre of the circulation and distributed through the body by the arterial blood. The other mode was when the contamination of neighbouring organs was the effect of immediate contact. Sanderson had described as the result of experiments, that when he introduced tuberculous matter into the pleura, tuberculous growths in time were found deep in the liver; whereas when he introduced it into the peritoneum the granulations were found on the surface of the liver, but did not appear in the centre of the organ, as it did when it reached the liver by the blood.—*Dec. 11, 1869.*

Necrosis of the Femur.—DR. BENNETT exhibited to the Society the left femur, and portions of the bones of both legs of a man who had died in Sir P. Dun's Hospital, from the effects of a severe injury. On the morning of the 30th November, the man, who was over 60 years of age, was knocked down by a dray, the wheel of which passed over both his legs. A compound and comminuted fracture of the right tibia and fibula was the result, and considerable bruising of the left thigh and other parts.

The man had been lame from boyhood; his left thigh was deformed, the thigh-bone being much enlarged; the left knee-joint had but a very limited range of movement, and there were many scars adherent to the bone on the posterior and lateral aspects of the limb.

There was considerable hemorrhage from the wound of the right leg at the time of the man's admission to the hospital, and his pulse was weak and intermittent. After a few days, the soft parts in the vicinity of the fracture sloughed, and the bones were exposed for a great extent. The condition of the limb was such that amputation below the knee was necessary, but was delayed until the week before Christmas, in consequence of the extreme fever and depression present. On December 18th, the left knee showed signs of inflammation; but it was difficult to determine the condition, in consequence of the deformity of the limb, and the ecchymosis present, resulting from the injury.

Up to the date of the amputation, the patient referred all his pain to the broken limb, and did not complain of the left thigh or knee. He stated that for his whole life, from the time of his boyhood, that limb was liable to inflame from slight causes; but that some of the scars on such occasions opened, and the occurrence of discharge from them relieved him.

On the 24th December, Dr. Bennett amputated the right leg, below the knee, and for a few days the patient improved slightly, and the stump did well. At the end of the first week, the pulse rose, a severe rigor occurred, the stump began to slough; and an abscess was opened behind the left knee, which evidently communicated with the joint. From this time the patient sank rapidly, and died on January 8th.

There was nothing of special interest in the condition of the parts removed by the amputation, nor in the stump. The examination of the left limb showed that recent inflammation had been set up in the knee-joint, which had been partially destroyed by fibrous ankylosis. In all probability, the ankylosis had existed during the greater part of the man's life, and the suppurative inflammation had been caused by the injury inflicted by the dray wheel. The muscles of the thigh were in great measure replaced by fat, and the soft parts were closely connected to the bone by numerous cicatrices. The lower part of the thigh-bone was much expanded, and contained several sequestra, one of which projected into the popliteal space, directly above the knee-joint. The surface of



the bone was rugged, and grooved on its inner side by the femoral artery. The projecting portion of the sequestrum was contained in a dense capsule of fibrous tissue, which separated it from the recent abscess connected with the joint. This capsule was continuous with the thick periosteum of the femur, and it was closely applied to the dead bone. The upper third of the femur was rough, and slightly enlarged, but there were no sequestra contained within it. The tissue of the upper portion of the shaft and of that containing the sequestrum was very heavy and dense. The condyles contrasted strongly with the shaft; their tissue was degenerated, and broke with the slightest force in front or behind; the only portions of them that retained their normal strength were the parts applied to the head of the tibia, which, in the ankylosed condition of the knee-joint, supported the weight of the body. The condition of the head and neck of the femur was most remarkable. The neck was a mere spur of bone, presenting on its upper surface a convex facet, which measured half an inch transversely by an inch antero-posteriorly; to the border of this facet, and to the under surface of the spur-like neck, three small pieces of bone were attached by ligamentous tissue. Two of them, not unlike the scaphoid and lunar bone of the carpus, completed, with the facet on the neck, the upper articular surface of the femur; the third piece was a mere spicula of bone, formed in the ligamentous tissue which connected the larger pieces to the neck and to each other. The surface of the acetabulum was like that of a joint affected by chronic rheumatic arthritis; it was shallower than normal, in parts eburnated and in others rough; no articular cartilage was present, either on its surface or on the articular surface of the femur. The condition of the upper end of the femur appeared to have been produced by the extension of the inflammatory attack which had involved the shaft of the bone, in early life, to the superior epiphysis; at least, the history of the case pointed to this explanation as the most probable; for the patient stated that his limb was free from any disease until he was fourteen years of age, at which time abscesses formed, and many pieces of bone came away from the thigh. The patella was light and spongy in texture, and was connected by fibrous tissue to the condyles. The upper extremity of the tibia was similarly wasted, and its surface marked by crusts of bone in the lines of attachment of the soleus and other muscles.

On section, an encysted abscess was exposed, occupying the greater part of the superior cancellated tissue of the bone. The cyst extended upwards nearly to the articular surface, but did not open at any point into the articulation; it extended downwards for two and a-half inches, along the junction of the posterior and internal walls of the bone.—*January 8, 1870.*

Cerebral Aneurism.—PROFESSOR R. W. SMITH made the following

communication to the Society; and exhibited a series of drawings, illustrative of the pathology of cerebral aneurism.

Notwithstanding the recent and valuable labours of English and Continental writers, I think it will be admitted that the diagnosis of cerebral diseases is still surrounded by considerable difficulty, and enveloped in much obscurity. We may be able to say in general terms that any given case is one of brain disease; we may, in many instances, go so far as to refer it to a particular class of these diseases, and even to localize the affection; but how many instances occur where we are utterly unable to pronounce a positive opinion respecting the nature or seat of the lesion. We can safely assert that a patient labours under the symptoms of compression of the brain; but how seldom can we tell with any degree of certainty the nature of the compressing force, or say whether it is an aneurismal tumour, an exostosis, a tumour of the dura mater, or in the substance of the brain, &c.!

It is to the first-named of these diseases that I am anxious to direct attention upon the present occasion, not in the hope of diminishing the difficulties that surround the diagnosis, but with the desire of endeavouring to make even a slight advance in our knowledge of the pathology of cerebral aneurisms.

Long before I had the advantage of reading the very valuable essay of Dr. Senhouse Kirkes on some of the effects of the detachment of fibrinous deposits from the interior of the heart,^a I was of opinion that aneurisms of the arteries of the brain admitted of being divided into two great classes, and that they should not be considered as originating, in all cases, in disease of the arterial tunics. I was, therefore, pleased to find a confirmation of this idea in the able memoir which I have mentioned. Dr. Kirkes observes—"In consequence of the obstruction thus produced, the blood, continually impelled up to the obliterated part, will naturally tend to induce distention of the coats of the vessel immediately behind the obstruction. And it may be a question whether many of the aneurismal pouches found in the cerebral arteries may not originate in this way. In favour of such a view may be mentioned the facts, that the origin of one or other middle cerebral artery^b is the most frequent seat of such aneurism; that they are commonly found uncombined with any disease in the coats of the rest of the cerebral arteries; that they are not unfrequent in young persons, in whom general disease of the arterial system is rare; and that in many instances they are found associated with valvular disease of the left side of the heart."

I feel satisfied that this conjecture is well founded, and that there are two forms of aneurism affecting the arteries of the brain, originating

^a "Medico-Chirurgical Transactions," Vol. xxxv., 1852.

^b The statistics of cerebral aneurisms show that the basilar is the artery most frequently affected.



MR. HAMILTON.—WOUND OF THE HEART.

from distinct causes, and to a certain extent differing not only in the effects they produce upon the cerebral structure, but likewise in the mode of invasion and progress of the symptoms, and frequently in the manner in which they terminate. The one may be called idiopathic,^a and the other may, I think, be justly termed embolic. The idiopathic originate in a pre-existing morbid condition of the arteries, most probably of a constitutional character; the embolic having for their cause the presence of a foreign body in the shape of a particle of fibrine detached from the valves of the left side of the heart and arrested in a *healthy* cerebral vessel. The embolic have a much more marked and a much earlier tendency to become solid, and consequently are slower to rupture than the idiopathic. The embolic are in all cases examples of true aneurism; while the idiopathic, there is reason to suppose, judging from analogy, are in many instances accompanied by a solution of continuity in the lining membrane of the vessel; whether this is due to a lesion of nutrition, or (as Dr. Bristowe believes) to a congestive and inflammatory process.

The observation has been often made, that all intra-cranial are true aneurisms, and formed by the uniform dilatation of all the arterial tunics. Such is the doctrine advocated by Hodgson, Albers, Cruveilhier, Gull, and other writers; but perhaps, as our knowledge of this obscure affection advances, it may be found correct to limit this statement to those cerebral aneurisms that can be traced to an embolic origin.

As regards their effects upon the cerebral structure of these two forms of aneurism, it would seem that in the embolic, softening is a very frequent result, and is of comparatively early occurrence; while in the idiopathic we rather look for laceration of the substance of the brain, produced by the rupture of the sac, and the consequent escape of the more fluid portion of its contents into the contiguous cerebral structure. The embolic aneurism in many instances, at all events, appears to be more sudden in its invasion, and more rapid in its course than the idiopathic, in which the patient may labour under symptoms which are those of compression, for a very considerable period before the fatal rupture takes place; indeed, in some cases death ensues without the sac having given way, the symptoms of compression continuing to the end without any sudden aggravation occurring.

CASES.—A man, aged about thirty-five years, was admitted into the Richmond Hospital on the 16th of November, 1836, having two days previously been found lying in the street, insensible, and in a dying state. None of the commemorative circumstances of the case could be ascertained from those who conveyed him to the hospital. At the period of his admission he was in a semi-comatose condition, from which it was

^a Of the idiopathic or spontaneous aneurism of the cerebral arteries, Gougenheim mentions three forms, viz.—true, mixed external, and dissecting. The occurrence of the last-named variety has been demonstrated by Kölliker and Virchow.

possible to arouse him for a moment, but he relapsed immediately. His breathing was extremely laboured, and accompanied by deep stertor; his pulse beat slowly and feebly, and his pupils were permanently dilated. There was incontinence of urine, and involuntary escape of the contents of the bowels. The left arm and both lower extremities were paralysed. When asked in a very loud voice, and repeatedly, if he suffered pain, he replied by raising slowly his right hand, and suffering it to wander, as it were, over the left side of his head. In this condition he lingered until the 21st, five days from the date of his admission into the hospital.

When the interior of the cranium was examined after death, a firm and perfectly solid tumour, of an oval form, and one inch and three-quarters in its antero-posterior diameter by one and a-half in its transverse, was found at the base of the skull, near the centre of the middle fossa; the greater part of it corresponded to the floor of the third ventricle of the brain; but its posterior portion was in contact with the lower surface of the pons Varolii, in which it had to a certain extent imbedded itself; the whole of the cerebral structure in contact with the tumour was of a tawny colour; but, although it did not possess its natural firmness, the alteration from its normal consistence was extremely slight indeed. The several parts composing the floor of the third ventricle were no longer distinguishable.

A section of the tumour (see Plate, Fig. 1) disclosed a mass of firmly coagulated blood, disposed in concentric laminæ, and to a certain extent deprived of colouring matter. The sac was completely filled by this coagulum (not a single drop of fluid blood being anywhere visible), and perfect throughout its whole extent. The aneurism appeared to have sprung from the posterior artery of the cerebrum, but the solid coagulum of the sac had completely occluded its orifice. It is obvious that in this case death was not owing either to softening of the brain or to rupture of the sac; and we are forced to ascribe it to the effects upon the brain of the pressure of a large, solid tumour, into which the aneurism had been converted by the coagulation of its contents. The case must further be looked upon as an example of cerebral aneurism that had undergone a spontaneous cure, yet, in consequence of its locality, proved fatal by pressure.

CASE.—The following may, I think, be considered as a typical example of embolic aneurism of the middle cerebral artery, having its distant source in disease of the aortic valves, and resulting in fatal softening of the cerebral structure.

A servant girl, twenty-six years of age, was under medical treatment in the Whitworth Hospital in the year 1857, for a cardiac affection, which commenced during an attack of rheumatic fever. In 1865, on the 15th of February, she was again admitted, in the following condition:—The physical signs indicated a slight degree of aortic patency; a faint

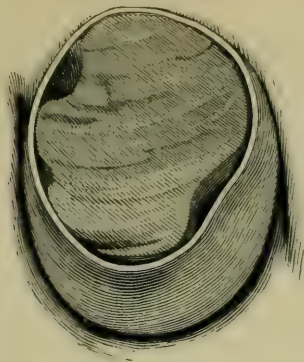


Fig. 1.

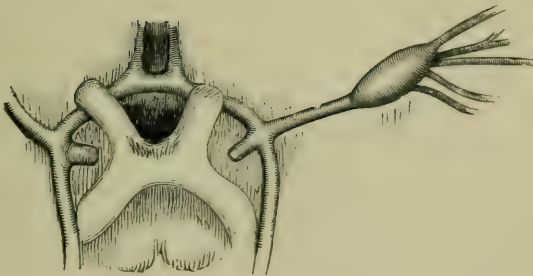


Fig. 2.

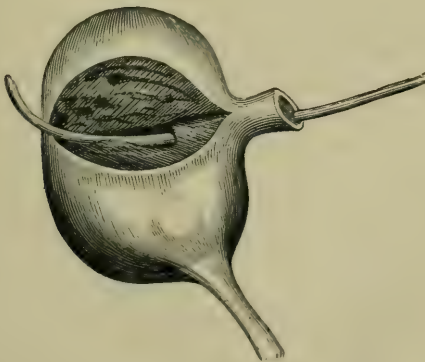


Fig. 3.

single diastolic murmur was audible over the region of the base of the heart, and at the upper part of the sternum, but was inaudible towards the apex of the organ. The first sound was everywhere normal. The pulse possessed a jerking character. Notwithstanding the long duration of the disease, there was no evidence of hypertrophy of the muscular structure of the heart. Moreover, she had never hitherto suffered from any symptom of cardiac distress, with the exception of once, about four years ago, when she was under treatment for palpitations and fits of syncope, apparently induced by her employment in a Turkish Bath establishment. These symptoms were soon relieved, and did not recur.

She complained very much of constant pain of a neuralgic character in the forehead, sometimes shooting down the face and neck; she suffered much from debility, and during the four months preceding her second admission into the hospital under Professor M'Dowel's care she had become thin and depressed in spirits. Her aspect was anemic, and her whole appearance indicated extreme delicacy. She had always been the victim of catamenial irregularities, and for six months before her admission had not menstruated. She remained constantly in bed, and evinced the utmost disinclination to make the slightest exertion; she gradually declined in strength, without any marked symptom, until the beginning of March, when she suffered from a smart attack of bronchitis, which, however, gradually yielded to treatment.

When visited, upon the morning of the 1st of April, she was found to be inarticulate, and hemiplegic on the right side. The features were drawn towards the left; there was little, if any, sensation on the paralysed side, and all the muscles were perfectly flaccid. Slight reflex actions could be excited in those of the sole of the foot. She seemed quite incapable of comprehending any question addressed to her; at all events, when spoken to, she only mumbled something that was perfectly unintelligible. She was obviously in pain, as she moaned continually, and often moved her hand to her head; her pulse was 144.

The night nurse reported that at 5 o'clock in the morning she asked for something, and was attended to. She had no attack of a convulsive character.

Upon the 3rd of April the biceps muscle had become somewhat rigid, and an attempt forcibly to extend the forearm at once caused her to cry out with pain. In other respects her general condition remained unaltered. She gave no indication by sign or otherwise of understanding what was said to her, or going on in her presence. She swallowed easily, but the bladder and rectum acted involuntarily.

In this melancholy state the poor girl struggled on until the 23rd: her emaciation became extreme, and large bed sores formed over the sacrum and right hip. Four days before her death the whole body, but more especially the paralysed side, became covered with myriads of purpuric

spots of various sizes, some of which sloughed. The biceps retained its rigidity, so that her arm remained constantly folded across her chest, and any attempt to extend it evidently gave her pain. She died this evening. Professor M'Dowel immediately after the paralytic seizure made the diagnosis of "Paralysis from the presence of an embolus in the left middle cerebral artery."

The body was examined thirteen hours after death. An unusually large amount of sub-arachnoid effusion existed in the meshes of the pia mater. The lateral ventricles likewise contained much fluid. Upon tracing the left middle cerebral artery into the fissure of Sylvius, it was found to be obstructed (just where it branches into twigs surrounding the island of Riel) by a plug of fibrine of a yellowish colour and oblong form, fully a quarter of an inch in length, and about the eighth of an inch in breadth.^a At the seat of obstruction the vessel was dilated into an oblong tumour, half an inch in length, and a quarter of an inch broad, the space intervening between the original plug and the arterial tunics being occupied by coagulated blood. The trunk of the vessel upon this side from its origin to the commencement of the tumour was smaller than its fellow, and the same remark applies to most of the arteries composing the left side of the circle of Willis. Just beyond the situation of the tumour, in the middle lobe, at the bottom of the fissure of Sylvius, there was an area of softened brain substance, about as large as a walnut. Several of the branches of the obstructed artery were traceable into it.

In the thorax the cavity of the pericardium was found to have been obliterated, the parietal and visceral layers of the membrane being connected to each other by a delicate but resisting areolar tissue, evidently of very old formation. A large, irregular, warty growth existed upon the edge and upper part of the most external sigmoid valve. The two remaining valves were thicker than natural, and attached to one another at their adjacent margins. Their edges were rough and serrated, looking exactly as if portions of fibrine had been detached from them. A large vegetation, similar to that on the aortic valve, existed on the aortic side of the septum of Lieutaud, the side looking towards the ventricle being quite smooth. All these vegetations or deposits were soft and friable, and of precisely the same colour and consistence as that found in the cerebral vessel. The lungs were healthy.

Numerous small purpuric spots were noticed on the surface of the internal organs, as the kidneys, uterus, intestines, &c.

The following case, although it is not an example of intracranial aneurism, is, I think, sufficiently remarkable, both as regards its history and its termination, to be worthy of being recorded upon the present occasion.

^a See Plate, Fig. 2.

It furnishes an instance of brachial aneurism of embolic origin, connected with disease of the left side of the heart, and (as far as relates to the aneurismal tumour) terminating in a spontaneous cure.

CASE.—A female, aged about thirty-eight, who had long suffered from cardiac disease, was admitted early in 1867 into the Whitworth Hospital, under the care of Professor M'Dowel, and was found on examination to be the subject of obstructive disease of the mitral valves. A detailed account of the signs and symptoms which guided to this conclusion it would be quite foreign to my present object to enter upon. Suffice it to say, that a short time after admission she was seized suddenly, and at night, with intense pain at the bend of the elbow of the right arm; and in the morning the dorsum of the hand was found to be œdematous, and the forearm swollen as high as the elbow, the pain continuing with severity; at the flexure of the elbow-joint, exactly in the usual site of brachial aneurism, when resulting from venesection, a circumscribed and very painful tumour, resembling a walnut in form and size, was visible: to the feel it was firm and solid; there was much swelling around it, and the skin showed a blush of redness. This condition of parts led at first to the idea that an abscess was in progress of formation. This impression, however, was soon seen to be incorrect; for upon a subsequent and more complete examination the presence of a distinct *bruit* was ascertained. It is true that at the first examination that was made of the tumour pulsation was detected, but it was supposed to be communicated to it from the brachial artery. For several days the tumour steadily enlarged; the pulsation (distinctly expansive) and the *bruit* were persistent; the pain became much more intense, and the œdematous swelling of the forearm increased. About this time Dr. M'Dowel kindly requested of me to see the case with him in consultation; and after a most careful examination, I came to the same conclusion that this accomplished physician had previously arrived at, viz., that the tumour consisted of a dilatation of the brachial artery, produced by the impaction of an embolous in its tube, just above the point of its bifurcation. I recommended him to abstain for the present from all operative interference, but that a cautious trial of pressure might be made; and I expressed my opinion that there was a chance of the spontaneous subsidence of the tumour.

It was, however, found that she could not endure the increase of pain which even a very moderate pressure with the instrument gave rise to; so that, after its employment at intervals for a few hours, it was found necessary to abandon its use; during its application, the pulsation in the sac was arrested, but there was no diminution in the size of the tumour, and this is an important fact in its bearings upon the pathology of aneurism traceable to embolic origin. Short as was the duration of the pressure, it seemed to have produced a beneficial effect; at all events, whether owing to this cause or not, the solidity of the tumour increased, the pain

diminished, the pulsation became less perceptible, and ultimately ceased. She was then, for the benefit of change of air, transferred to Sir Patrick Dun's Hospital; and after a short sojourn there, she went home, the aneurism, it may be said, cured; the tumour was much diminished in size, perfectly solid, painless and pulseless. The affection of the heart was, however, gradually and steadily progressing.

After remaining at home for a few weeks, finding herself getting worse, she again applied for, and obtained admission into the Whitworth Hospital, where the cardiac affection soon terminated her existence.

The *post mortem* examination disclosed the existence of an extreme degree of contraction of the mitral orifice, with friable fibrinous depositions upon the thickened valves. The brachial tumour was removed, with the arteries above and below it; but owing to the illness of Professor M'Dowel at the time, a sufficiently accurate examination of it was not made; enough, however, was done to show that a spontaneous cure had taken place. The tumour was not as large as a shelled almond, which it resembled in form, and was perfectly solid throughout. The obliteration extended upwards for a very short distance along the trunk of the brachial artery. In Mr. Tufnell's case, recorded in Vol. XV. of the *Dublin Quarterly Journal*, and which was perfectly analogous in all its leading characters, the obstruction and consequent tumour were situated in the popliteal artery.

In 1854, the late Dr. Mayne laid before the Pathological Society a specimen of cerebral aneurism, which proved fatal, by rupture of the sac, four years after the first occurrence of head symptoms. As the case has never been published, and as the patient was under Dr. Mayne's notice during the entire of those four years, and was carefully observed, I now place it on record.

CASE.—A coal-heaver, thirty-three years of age, was brought into the Hospital of the South Union, early in the month of April, 1850. He had (like most of his class) been greatly and constantly addicted to drink, and he had also led a most laborious life.

For three days before his admission, he had gone through great fatigue in unloading a coal ship. On the evening of the third day he got drunk, lay out all night, exposed to the weather; and on attempting to rise next morning, he staggered and fell, and was carried to the hospital in a state of complete insensibility.

When seen by Dr. Mayne, he appeared to labour under profound apoplexy; his breathing was stertorous, his head and face congested; the surface was cold, and there was retention of urine, and inability to swallow.

Under appropriate treatment, he gradually regained his consciousness; the power of swallowing returned; the bladder, after catheterism for three weeks, regained its tone; and he finally recovered sufficiently to be

able to leave the hospital, but with very considerable impairment of the functions of the brain remaining. He was listless and stupid, and his memory was nearly gone.

The only positive paralytic symptom which remained was ptosis of the right eyelid—not, however, absolutely complete, for he could raise the lid by a voluntary effort sufficiently to expose the greater part of the cornea; but, except on occasions of great effort, the eyelid drooped so as altogether to conceal the eye-ball. When it was raised by extraneous force, the cornea was found to be turned outwards, and slightly upwards, in a state of divergent strabismus.

There was, also, a very considerable amount of amaurosis; for, although large objects could be distinguished by the right eye, when the lid was elevated, they were very obscurely visible, and small bodies could not be seen at all.

The tactile sensibility of the conjunctiva, and of the right side of the face, was unimpaired; hearing was not affected, nor was there any trace of facial paralysis.

The man remained in one of the workhouse wards during a period of four years, without any very marked change in his symptoms. At length he suddenly fell, in an apoplectic seizure of great severity; his breathing became stertorous, severe convulsions attacked him; and he died, comatose, in a quarter of an hour.

The case had been so long under observation, that the greatest interest attached to the *post mortem* examination. The existence of a cerebral aneurism was not suspected; but, from the occurrence of ptosis and divergent strabismus, it was evident that the lesion, whatever was its nature, had involved the third nerve, and the amaurosis indicated that the optic nerve was in some way implicated.

There was venous turgescence of the scalp. On removing the dura mater, an extensive effusion of semi-coagulated blood was found covering the outer and anterior portion of the right hemisphere.

The interior, also, of this hemisphere contained an immense coagulum, which had extensively torn up the cerebral structure, and forced its way into the lateral ventricle.

On removing the brain, and examining its inferior surface, the source of the hemorrhage was found to be an aneurismal tumour, about the size of a small plum,^a situated at that part of the circle of Willis where the internal carotid artery terminates. It was almost completely filled with laminated coagula, of great density, some of them of a buff colour, and leathery consistence. The sac had given way at its upper and outer part by a rent, through which a probe was readily passed. A torrent of blood had in this way escaped, and torn up the brain from below,

^a See Plate, Fig. 3.

causing the profound sanguineous apoplexy which had proved so suddenly fatal.

The right third nerve was amalgamated with the tumour so closely, that the scalpel failed to separate them; it had lost its natural colour, seemed stretched, and was flattened by pressure. To this condition of the nerve must be ascribed the ptosis and divergent strabismus.

The optic nerve was soft, flabby, and of a dingy colour, and presented in every respect a different appearance from that of the opposite side; and to this altered state the amaurosis is, no doubt, to be referred.

In the preceding account of this most interesting case, no mention is made of the occurrence of cardiac symptoms at any period of its progress, from which it is to be inferred that none such existed; for, had they been present, it is impossible they could have escaped the notice of so careful and so able an observer as Dr. Mayne is well known to have been. We cannot, therefore, place this case in the rôle of those produced by the presence of an embolus of cardiac origin in a cerebral artery, but must rather look upon it as the result of arterial disease, induced by long-continued and inveterate habits of intemperance.

In his observations on the preceding case, Dr. Mayne expressed his surprise that the first symptoms of a cerebral aneurism should have been those of profound apoplexy. But it must be borne in recollection that, in 1850, when this case came under his notice, the extent of our information respecting cerebral aneurism and cerebral hemorrhage was not by any means as great as it is at the present day. This advance is due to the labours of Kirkes and Virchow upon the subject of embolism—to the valuable reports of cases of cerebral aneurism published by Drs. Brinton, Gull, Ogle, and other English authors, and to the researches into the entire subject by Lebert and Durand, and above all Charcot and Bouchard, whose elaborate investigations respecting miliary aneurisms have thrown much light upon the subject of cerebral hemorrhages. These miliary aneurisms frequently co-exist with aneurism of the larger cerebral vessels; and it is possible that the first attack of cerebral hemorrhage, in Dr. Mayne's case, was due to the rupture of one of the former—the fatal apoplectic seizure being the result of the giving way of the aneurismal tumour, found after death. The complete absence of symptoms previous to the sudden apoplectic seizure is by no means uncommon, but we do not often find the duration of life after this, so prolonged as it was in this instance. Ollivier has recorded three cases of aneurism of the basilar artery (taken from the writings of Hodgson, Serres and Lebert), in two of which there were no symptoms till the tumour gave way, death rapidly ensuing with apoplectic phenomena. Not so, however, in the third case. In this there was, in 1817, a sudden attack of unconsciousness and paraplegia. The patient soon recovered his senses; but the paralysis persisted, and extended upwards, involving

the upper limbs, and causing dyspnœa, dysphagia, and feebleness of voice. For years these symptoms made little progress, and the patient was able to walk with the aid of a stick. He died in 1835, having survived for eighteen years after the first setting in of the symptoms.

The aneurism (which had not burst) compressed the anterior part of the medulla oblongata, as well as the origins of the glosso-pharyngeal, pneumo-gastric, and ninth-nerves.—*January 22, 1870.*

Disease of the Tricuspid and Mitral Valves.—DR. CRYAN exhibited a specimen of valvular disease of the heart engaging both sides of the organ—namely, extreme narrowing of the left auriculo-ventricular opening, with calcification of the mitral valve, and also great constriction of the tricuspid opening, both orifices allowing regurgitation. He said that, as he had ventured to make the diagnosis of this double lesion during the lifetime of the patient, and eight days before her death had announced it and the data on which it was founded to the members of the hospital class, and to some physicians who happened at the time to favour him with a visit to his wards, he might, perhaps, be permitted to refer to the clinical history of the case. The subject of it was an unmarried woman, aged 26, rather below the middle height, but of tolerably stout build. She was admitted into St. Vincent's Hospital on the 15th of January, 1870. She was a general servant, and stated that she had usually enjoyed good health till fourteen months before her admission. She then suffered from a severe attack of acute articular rheumatism, for which she was admitted into St. Vincent's Hospital, under one of his colleagues. She was discharged, apparently quite well; and they had no direct evidence that she laboured, while in hospital, under any cardiac affection. Very soon after leaving she began to complain of palpitation, shortness of breath on making the slightest exertion, and occasional dizziness. Six months later, after a fright, the catamenia were suddenly suppressed and had never re-appeared.

She now began to suffer from short cough, and, for the first time, observed some swelling about the ankles; the difficulty of breathing became more distressing; the expectoration contained small quantities of blood; and, now and then, she became faint, and had "noises in the head" on suddenly assuming the erect posture. She was now admitted into one of the hospitals in this city, and left it after a fortnight without much relief to her symptoms. On admission into St. Vincent's Hospital, on the 15th of January, the face was livid and swollen, and wore a very anxious expression; the eyes were red and suffused, the pupils rather large, but equally dilated, and readily responded to the stimulus of light; the lips were blue, the general surface cold; she complained of extreme weakness, and that she had not had any sleep for the three previous nights; she was affected with severe cough, with copious sero-mucous expectoration,

dyspnœa, occasional orthopnœa, and the lower extremities were livid, cold, and largely anasarcaous, and a few dark-coloured bullæ were visible on the dorsum of each foot.

Both upper extremities were also œdematous, the right in a greater degree than the left. She habitually inclined to that side. The front of the chest was clear on percussion, except over the region of the heart, and moist bronchitic *râles* were audible over the greater part of it.

The extent of cardiac dulness was much increased, measuring nearly four inches in a transverse direction, this increase being chiefly towards the right; the impulse of the heart, which extended into the epigastrium, was strong, but not heaving; the action quick, irregular, and occasionally intermittent; the apex, over which there was slight fremitus, beat in the sixth intercostal space, and was also displaced outwards, pulsating opposite to a line let fall from the left nipple. At the left apex two murmurs were distinctly audible; one was soft, prolonged, and presystolic, running up to—but not extending into—what should have been the ordinary first sound; the other murmur was systolic, rather rough and loud, corresponding to the ventricular contraction; the healthy second sound was weak, but yet audible at the apex of the heart. There was no murmur over the base of the heart, or in the inter-scapular region; but, at the junction of the fifth costal cartilage with the sternum, a soft systolic murmur was audible; this murmur became gradually fainter as the stethoscope was moved towards the left edge of the sternum, where it ceased to be heard. Over the site of the pulmonary artery, in the second left intercostal space, both sounds of the heart were loud and distinct; the second sound was unusually strong and clear, and there was no reduplication of it. At the right edge of the sternum, corresponding to the second intercostal space, over the site of the aorta, both sounds were remarkably feeble. There was well-marked dilatation with visible and tactile pulsation of the jugular veins—particularly the right; this pulsation was single, did not disappear when the respiration was momentarily suspended, and was synchronous with the ventricular systole. There was no visible pulsation of the arteries of the neck. *There was no pulse to be felt in the right radial artery*; in the left it was small, thread-like, irregular, and occasionally intermittent, about 100 in the minute. The radial pulsations were about ten less in the minute than those of the heart. The respirations were shallow and painful, 38 in the minute. Temperature in the axilla, 99° F., on the dorsum of the foot only 64° F.

Small muco-crepitating *râles* were audible over the greater portion of the right inferior-dorsal region, while on the left there was bronchial respiration, with dulness on percussion, extending nearly as high as the inferior angle of the scapula. The right lobe of the liver projected two fingers' breadth below the false ribs.

The patient complained of thirst; the tongue was large and livid, but

clean and rather moist; urine scanty and turbid; appetite bad. Dr. Cryan said that, combining the physical signs in this case with the general symptoms and history, he made the following diagnosis:—Narrowing of the mitral orifice with regurgitation; regurgitation through the right auriculo-ventricular opening, *probably* from its dilatation; congestion of both lungs, with consolidation of the left inferior lobe. On the 16th, under treatment directed to relieve the congestion of the lungs, the patient felt greatly improved, and had enjoyed some hours of tranquil sleep; the physical signs were without notable change; the urine very scanty—about 8 ounces—acid, high-coloured, loaded with urates, and containing a moderate amount of albumen, but no renal casts; Sp. gravity, 1027. For the next four days the general symptoms and physical signs remained unchanged; the patient complained of pain in the right foot, but the gangrene had not extended. On January 20th there was dyspnoea, with frequent cough, the expectoration deeply coloured with blood; pulsation still absent in the right radial artery, and in the left it was small, extremely weak, and irregular. Both the presystolic and systolic murmurs were, at times, quite inaudible.

January 21st.—Patient passed a better night, and to-day the dyspnoea is not so urgent; both murmurs are distinctly audible over left apex; systolic murmur at tricuspid orifice, also distinct; pulse in left radial artery.

January 22nd.—Passed a sleepless night. Great dyspnoea and lividity. *No pulse to-day in either radial artery*; the fremitus at the apex of the heart has also disappeared, and the cardiac sounds are so feeble as not to admit of any analysis; anasarca extending to genitals and abdomen; temperature in axilla 98° F.; on dorsum of foot 69° F.; urine extremely scanty; intellect quite clear, but the patient is evidently sinking.

On the morning of the 23rd January, the patient, after drinking a few spoonfuls of beef-tea, complained of a feeling of faintness; the face assumed a livid pallor, and, in a few moments, without spasm or convulsion, she passed into a state of syncope, in which she expired.

On *post mortem* examination, both lungs were found intensely congested; and the lower lobes, particularly the left, presented several dark, hard nodules of pulmonary apoplexy, varying in size from that of a pea to that of a large nutmeg. The heart lay transversely in the thorax. On opening the pericardium, its cavity was found to contain about one ounce of reddish serum; and the inner surface of the serous layer was opaque, rough, and thickened, and mottled over with small oval patches of old lymph, of a greyish-white colour, affording ample evidence of a former pericarditis. There were only a few short, thread-like adhesions to the heart. The heart weighed 10½ ounces; its figure was somewhat globular, the apex being chiefly formed by the right ventricle. On opening the left ventricle, its cavity was found to be at least one-fourth smaller than

natural, and to contain two or three small red coagula; the ventricular wall was firm and red, and measured, at the junction of its upper and middle thirds, $5\frac{1}{2}$ lines in thickness; the two papillary muscles were shortened, and much atrophied; the mitral valve was thickened, opaque, and indurated from calcareous degeneration; its segments were greatly narrowed and shortened, and the chordæ tendineæ were rigid, and crowded together around the much-constricted and roughened mitral opening, which, viewed from the ventricle, was oval in outline, and measured rather less than three lines in its long axis; the mitral orifice, when viewed from the auricle, presented the well-known crescentic fissure—the convexity directed forwards, and to the left—and was so contracted as not to admit the tip of the little finger. The left auricle, which contained several dark-red coagula, was considerably dilated, its walls hypertrophied, measuring $2\frac{1}{2}$ lines, the lining membrane thickened, and of a pale buff colour; the pulmonary veins were dilated to nearly double their normal size; the right ventricle was moderately dilated and hypertrophied; the wall, at its base, measured 3 lines; the tricuspid opening was so contracted as to be able to receive only the tip of the index finger; the segments of the valve were shortened, thickened, opaque, and so rigid that the opening which they encircled must have remained patent, and permitted free regurgitation during life; the calcareous degeneration was not so far advanced in them as in the mitral valve. The right auricle was hypertrophied, and greatly dilated; it was distended with dark-red, soft coagula; and the openings of the cavæ and that of the coronary vein were of great size. The arch of the aorta and the great vessels springing from it were very small, measuring little more than half their normal size; but their coats were healthy, with the exception of one or two small atheromatous patches in the lining membrane of the arch; the aortic valves were sound, and found, when tested in the usual way, not to permit of regurgitation. The venæ cavæ and the jugulars were greatly dilated; the vessels of the extremities were free and pervious. Microscopic examination showed no trace of fatty degeneration of the muscular tissue of the heart; the coronary arteries were healthy; the liver and kidneys were greatly congested, but in other respects quite normal. Dr. Cryan said, he thought this case worthy of record, not merely as showing in how brief a time cardiac inflammation may lead to fatal degeneration of the valves, or how long life may be compatible with extreme narrowing of both the mitral and tricuspid openings, but mainly when considered in relation to the diagnosis of valvular disease *existing simultaneously on both sides of the heart*. Regarded from this point of view, he might observe that the diagnosis in the present case was based on the following considerations:—

1. That we could distinguish two seats, or centres, of murmur, one at the left apex of the heart, and the other at the junction of the fifth right costal cartilage with the sternum.

2. That over the former centre two murmurs were audible; the first, soft, prolonged, and presystolic, occurred at the end of the long pause of the heart, and was caused by the stream of blood injected from the left auricle into the ventricle; this murmur was not propagated into the aorta, and—when taken in combination with the weak, thread-like, irregular, and intermittent pulse at the wrist; the strong tumultuous action of the heart, the dyspnœa, anasarca, and the signs of marked pulmonary congestion—was diagnostic of narrowing or stenosis of the mitral orifice.

3. That the second murmur was systolic, and rather rough in tone, having its point of maximum intensity at the left apex, not audible in the aorta—and caused by the jet of blood propelled by the left ventricle into the auricle; and was—when taken in conjunction with the character of the pulse, and the signs and symptoms of pulmonary and general venous congestion—diagnostic of mitral regurgitation.

4. That at the second seat of murmur, namely, the junction of the fifth right costal cartilage with the sternum, a ventricular systolic bruit was present; it diminished in intensity when the stethoscope was moved towards the left margin of the sternum, where it ceased to be heard; it was not audible over the aorta, or pulmonary artery; and this murmur, taken in combination with the distinct pulsation, synchronous with the ventricular systole, that was not only seen, but *felt* in the dilated jugulars, and remembering the large congested liver, and the general dropsy, seemed plainly to indicate free regurgitation through the tricuspid opening. Dr. Cryan said, it was instructive to observe that, notwithstanding the contraction of the tricuspid valve, there was an entire absence during life of any presystolic murmur to indicate that lesion.—*February 12, 1870.*

Mitral Valve Disease.—DR. CRYAN said, that a married woman, aged forty, and the mother of nine children, was admitted into St. Vincent's Hospital, under his care, on the 13th of October, 1869, labouring under many of the symptoms of heart disease. Her history was, that she had usually enjoyed tolerably good health until nine months before admission, when, after exposure to rain and cold, she became affected with a frequent dry cough, and on even slight exercise suffered from palpitations of the heart, and shortness of breath. She never spat blood, or laboured under rheumatism, or syphilis, or sustained any injury over the pre-cordial region.

On admission, the cheeks and lips were livid; there was marked turgescence of the external jugular veins, but no pulsation of them, and the lower limbs were cold and anasarcaous; she complained of vertigo, cough, with scanty mucous expectoration, and severe dyspnœa, with palpitation of the heart.

The front of the chest gave its normal resonance on percussion,

except over the cardiac region, where the transverse area of dulness was increased one inch; the impulse of the heart was strong, its action quick and irregular; there was no fremitus over the apex of the organ, which occupied its normal position; a loud systolic bellows' murmur replaced the first sound; this murmur was not audible in the interscapular region; there was no *bruit* at the base of the heart, but both its sounds were weak, and rather muffled, over the aortic valves; there was slight visible pulsation of the arteries of the neck; the pulse at the wrist was small, weak, and occasionally intermittent, about 110 in the minute; respirations, 40.

There was no abnormal dulness on percussion over the back of the chest, but muco-crepitating *râles* were audible over the entire of each inferior dorsal region; the liver projected fully an inch below the right false ribs; the urine was scanty, not albuminous, but loaded with lithates.

The diagnosis announced was, "regurgitation through the mitral orifice, and great congestion of the lungs and liver." Under treatment directed to relieve the pulmonary congestion, the more urgent symptoms subsided in a few days; and the patient was able to walk about the ward, and, at night, to obtain a fair amount of sleep. There was no remarkable change in her condition until the morning of the 25th of October—twelve days after admission to hospital—when the cough was noticed to be more frequent and distressing; and, about noon, she was seized rather suddenly with symptoms of incomplete aphasia; and when visited, about an hour after, the face had a peculiar livid pallor; the general surface was moist and cool; the pulse at the wrist, small, weak, and a little irregular; but it is to be borne in mind that it presented similar characters since the day of the patient's admission; the cardiac signs remained unchanged, and the mitral systolic murmur was still audible at the apex.

The patient was unable to utter certain words by which to express her thoughts, though she could clearly indicate her wants by gestures; for example, when she desired to ask if she might be permitted to leave her bed, or sit in a certain chair, or eat an orange, she would convey readily her wishes by pointing to these objects, and by other gestures; but every attempt to express them perfectly by words signally failed. Even when the words for which she was at a loss were pronounced slowly and audibly in her presence, she could not repeat them. I wrote the word orange, the one she was evidently at a loss for at the moment, to test whether its written symbols would recall that word, and enable her to utter it, but found that she had unfortunately never learned to read. Her inability to utter words was more especially marked in respect of nouns, or names of objects; she never applied words wrongly; the intelligence was little, if at all, impaired, the patient appearing to

understand readily everything said to her ; when asked whether she had pain in the head, she answered "No," and that this was not a stereotype reply to all inquiries I took care to convince myself.

There was no right hemiplegia, or other form of paralysis ; the movements of the tongue, lips, and soft palate were perfect, and the pupils natural.

For the next eight days the mental state of the patient remained unchanged, at the end of which period the aphasic symptoms disappeared rather suddenly, and she regained perfect power over articulate speech, and was no longer unable to utter any word as she might desire. Meantime the symptoms of the heart affection were fatally progressing ; the anasarca had increased ; the expectoration contained a considerable amount of blood ; and the patient suffered from frequent attacks of orthopnoea, and gradually lost strength, and expired on the 27th of January—seven weeks after she had regained perfect power of speech, which she retained to a few hours before her death.

Autopsy, sixteen hours after death :—The heart weighed $10\frac{1}{2}$ ounces ; the walls of the left ventricle were a little hypertrophied, but its cavity was of normal size ; the left auricle was dilated and hypertrophied, its walls measuring rather more than two lines in thickness ; the mitral orifice was contracted about one-third, its valve shortened and opaque, with the edges thickened ; water poured into the ventricle passed freely into the left auricle, the orifice of the aorta being compressed with the fingers, so that it was obvious free regurgitation was permitted during life ; the aortic valves were very slightly thickened, and a vegetation, about the size of a snipe-shot, projected from the free edge of one of the segments ; when tested by the water-cock, no regurgitation was permitted ; there was no appearance to indicate that any vegetation had been detached from either the aortic or mitral valves ; the orifices and valves on the right side of the heart were quite healthy ; the right auricle was a little dilated and hypertrophied, but the right ventricle had undergone no change ; the pulmonary veins were nearly double their usual size.

On a microscopic examination, the muscular tissue of the heart showed no sign of fatty degeneration. The lungs were much congested, and the base of the left contained two or three small nodules of pulmonary apoplexy. The kidneys were rather small, and much congested, but in all other respects quite healthy. Owing to the aphasic symptoms present during life, the brain, its blood vessels, and membranes were examined with much care, and were found to be perfectly healthy ; the anterior lobes, including Broca's convolution, the corpora striata, optic thalami, and the medulla oblongata were examined both microscopically and by sections, &c., with especial care, but presented no morbid change whatever ; there was no embolus in any of the large cerebral arteries, no

plugging of the minute vessels; the brain weighed forty-three ounces, and nowhere showed any trace of softening; the ventricles were free from serous or other effusion.

Dr. Cryan said he thought the chief points of interest in the case were—first, that it afforded an example of a patient, in whom the symptoms of partial aphasia were present for eight days, and then completely disappeared, leaving her intellect and speech perfect up to the period of her death—at the end of seven weeks—and in whose brain *post mortem* examination failed to discover any morbid change to shed light on the aphasic symptoms; and hence suggesting the inference that such symptoms may be caused either by temporary and circumscribed cerebral congestion, or by small, or capillary hemorrhages, leading, it may be, to defective nutrition of certain portions of the brain, but yet admitting of complete recovery, and leaving, after death, no trace of their previous existence.

Again, it is worthy of note that the patient's inability to utter certain words was not a mere amnesic effect; that, in fact, the case was not an example of the amnesic aphasia of Dr. Saunders, or the amnemonic aphasia of Dr. Ogle, inasmuch as the patient, when prompted, was unable to recall the word she required, and could not repeat it, though but just pronounced slowly and audibly in her presence. It belonged rather to those forms of defective expression, where the words, present to consciousness, yet cannot be articulated, owing (to adopt the language of Dr. Bastian^a) to some defect "in the nervous mechanism, by means of which the primary motor impulses (reflected from the revived auditory impressions) are transmitted downwards, from the grey matter of the cerebral hemispheres, to the more immediate motor centres for speech—these, perhaps, being partly situated in the corpora striata and partly in the medulla oblongata."

"If this passage or line of transmission is imperfect, then, although thought may proceed in a more or less perfect manner, it is not possible for the individual properly to translate his thoughts into acts of articulate speech." With regard to the mitral valve disease, it is to be noted that the left auriculo-ventricular orifice not only permitted free regurgitation, but was also contracted at least one-third, and yet repeated examinations failed to detect any presystolic murmur—a ventricular systolic *bruit*, at the left apex, being the only murmur audible during life.—*February 19, 1870.*

Caries of the Temporal Bone; Abscess in the Brain.—DR. JOHN HUGHES communicated the following case:—The specimen I have to show the Society is one of circumscribed abscess of the brain, connected with caries

^a Brit. and For. Med.-Chir. Review, Jan., 1869.

of the temporal bone—a disease of which there are, no doubt, many cases recorded, but still one sufficiently rare to be interesting.

My patient was a very well nourished, healthy, and good-looking girl, named Esther Murphy, twenty-two years of age, who lived as servant with a family in Kingstown.

In the month of August last, it was part of her duty to bathe the children of the family; and while so engaged, she caught cold (she believed) from staying too long in the water. This exposure was followed by a febrile attack, accompanied by pain in the head, and in *the right ear*.

The fever subsided after some days; but the pain in the head and ear continued (with occasional remissions), till the month of December, when all the previous symptoms became greatly aggravated, and another febrile attack ensued (with vomiting), and resulted in the formation of matter in the ear, and its escape through the external meatus. A discharge of fetid pus continued for three weeks, and gave great relief to the pain in the head.

In the month of January the purulent discharge ceased, and the pain in the head returned, with great intensity; however she endured it for *three weeks*, at the end of which period she was obliged to come to hospital, having discharged her duties as a domestic servant, up to the day before her admission.

As already stated, this girl appeared healthy, and told us she never had any illness until the present one, and that the catamenia were always regular. She did not remember whether she had scarlatina or measles, but was quite sure she had no sore throat, nor discharge from the ear, before last autumn.

I was in the hospital at the time of her admission, and my first impression on seeing her was, that she was ill of typhus fever. She looked dull and stupid, felt weak, and unable to assist herself; she could not stand or walk without assistance; and even when made to sit up in bed, she rocked to and fro like a drunken person. She was very deaf, and answered questions slowly, and complained of pain in the back of the head and neck, and behind the right ear. There was no discharge from the ear, but she was completely deaf on that side, and the mastoid portion of the temporal bone was very tender on pressure.

There was no impairment of the special senses, except that of hearing; no paralysis, either local or general; no delirium; no convulsion. The constitutional symptoms were, however, very severe. She had little appetite, and whatever she did take the stomach quickly rejected. The pulse was 110, very small, and weak; tongue coated with white fur; bowels confined; urine scanty and high-coloured; she had had no sleep for some nights.

The night after her admission the vomiting was incessant, and next

morning we found a large basin filled with a green-coloured fluid, which was intensely acid. The headache was rather worse.

Under treatment—namely, local bleeding and purgatives—the pain in the head abated and the vomiting was less frequent; and she seemed better for a few days, having had some sleep.

But, on the 8th of February, we found the pain of the head increased. She had no sleep; vomiting more frequent; some delirium during the night; complains of pain in the back of the neck and down the spine; breathing short and panting, with great apparent distress; pulse 108, small and weak; tongue coated with a white fur; pupils natural, but she dislikes the light; urine scanty, with a copious deposit; bowels moved three or four times during the night.

In the evening of the same day she had great heat of skin, with flushing—a very quick pulse, and restlessness—but there was no vomiting during the day.

9th.—The catamenia appeared during the night (a week before the proper time), and she feels much better this morning. She slept some, and perspired. There was no vomiting, and the pain in the head is greatly abated.

10th.—Had a good night; no headache, no vomiting; menses continue to flow; pulse 80; bowels free.

13th.—Continues relieved from pain of head and vomiting, but has become very weak. The pulse has fallen to 60, and is small and compressible. There is a purulent discharge from the ear, and a patch of herpes around the mouth, on the left cheek, and lobe of the ear.

15th.—Pulse has fallen to 50, weak and small; pain of head returns occasionally; sleeps pretty well. No vomiting nor nausea; no appetite for food or drink. She lies quietly on the left side; answers questions slowly, but quite correctly. She is, however, very low and desponding, and asks if there is any chance of recovery.

16th.—Remained in the same state during the day, and up to 9 p.m., when she became gradually comatose, and died at 3 p.m., to-day, fifteen days since her admission. She lay for eighteen hours like one in a quiet sleep, breathing easily, and without stertor, the pulse being very rapid; there was no convulsion.

I have said nothing of treatment in this case, although many remedies, both local and general, external and internal, were tried, because all were unavailing, I might say even to relieve symptoms. But one incident deserves to be noted here, namely, the signal relief from pain and vomiting, and the apparent arrest of the progress of the disease (which they marked), during the catamenial flow.

This patient lost much more blood artificially, than could have been furnished through the natural channels during the menstrual period, but without any relief to the most prominent and distressing symptoms; yet,

when this natural function was established, they almost marvellously disappeared.

The diagnosis was made with some little confidence, the condition of the ear leading to the inference that the disease had extended to the cerebral organ.

A *post mortem* examination was made six hours after death. On opening the cranium, the cerebral sinuses were found extended with dark blood. The membranes were rather more vascular in appearance than natural. After the removal of the brain, a small part of the dura mater, covering the petrous portion of the temporal bone, was found to be separated from it, and bulged up. This patch was reddish coloured, about the size of a sixpence, and situated above the tympanic cavity. On detaching the membrane from the bone, the latter was found to be perforated by a large aperture, communicating with the cavity of the tympanum, which was full of semi-fluid, cheesy-looking pus. The opening was about half an inch in diameter; the membrane covering it was red, thickened, and pierced with a small hole, which was only made apparent by injecting water under the membrane. The petrous bone was removed, and a section made, which showed destruction of the membrana tympani and bones of the ear. Two small red papillæ were noticed, growing from the side of the canal near the meatus externus.

A horizontal section of the brain was made about half an inch above its base, which disclosed a cavity filled to distention with disintegrated brain substance and greenish-coloured pus. This cavity was oval in shape, about two inches long by one and a-half wide, an inch deep, lined by a membrane, and found to communicate with the tympanum by an aperture corresponding with the perforation of the dura mater.

The portion of brain substance which, *in situ*, formed the floor of the cavity was very thin, and collapsed when the contents of the cavity were allowed to flow away.

There is nothing in this case different from those already recorded; it strengthens the already noted fact, that disease of the tympanum is connected with abscess in the middle lobe of the cerebrum, while disease of the mastoid cells is found to implicate the cerebellum, or to cause thrombosis in the cerebral sinuses. Although some of the most prominent symptoms of abscess of the brain—such as epileptiform seizures, paralysis, and delirium—were absent, still those usually found were present—namely, pain, vomiting, and stupor, or drowsiness.—*February 19, 1870.*

Ossification of the Choroid Coat of the Eye.—MR. WILSON said, that the man from whom the specimen was removed, contracted gonorrheal ophthalmia about five weeks previous to his coming under Mr. Wilson's care, and during that time he suffered from sleeplessness and agonizing pain, notwithstanding the treatment adopted by Dr. Kirkpatrick, to whom

Mr. Wilson was indebted for having transferred the case from the Work-house Infirmary to St. Mark's Hospital.

The right eye, which had been attacked, presented a densely leucomatous and flattened cornea, with a dark, brownish mark, showing where the globe had apparently burst, and the iris become adherent. To relieve the pain as well as preserve the second eye, the cornea of which was already nebulous, being attacked with sympathetic irritation, Mr. Wilson proceeded at once to enucleate the right eyeball. On removing the globe, it was so hard that it was at once pronounced to be filled with bone. On verifying this, and interrogating the man, it was ascertained that the eye had been blind since an attack of measles which he had had in infancy, about twenty years previously. On removing the sclerotic, there was scarcely a vestige of choroid at the posterior pole of the eye, but anteriorly this coat was to a considerable extent present. The optic nerve was represented by delicate filamentous remains of its cellular tissue, which passed in through the sclerotic and into a hole in the bony growth. The whole interior of the globe, from the position of the ciliary body backwards, was filled with a bony cup; posteriorly this bone was in contact with the sclerotic, was of a whitish colour, and presented an opening one line in diameter for the optic nerve, and minute openings for the ciliary nerves; anteriorly the margin of the cup was very irregular, terminating here and there in spicula. The cup was filled with new matter—the string-like remains of the retina, and pus. The lens was particularly cartilaginous, the cornea and iris were so intimately connected that it was impossible to separate them; the position of the ciliary body was occupied by neoplastic structure, about double the size of the ordinary ciliary body, and the bony growth commenced at this position.—*February 19, 1870.*

Wound of the Heart.—MR. HAMILTON detailed the following case, and exhibited the specimen. A man was brought to the Richmond Hospital who had been shot a few minutes before. He was walking with some brother Fenians, it was supposed, and was shot with a revolver in the chest by one shot, and in the upper and inner part of the right thigh by a second. After receiving the wounds, he cried out, and ran across the street, about four or five yards, and then fell. When he was brought to the hospital, he was quite dead. A wound, small and round, was found in the front of the chest over the heart, between the fifth and sixth ribs. Before it had reached the skin it had passed through a thick dreadnought coat, a woollen vest, a woollen comforter, and thick calico shirt. There was a round wound in the upper part of the thigh. Scarcely any blood had escaped from either. The course of the ball which had penetrated the chest, was first carefully followed. After having entered the chest, it penetrated the lower part of the right auricle in front; it then passed

through the upper and back part of the right ventricle, took an oblique course to the left, coming out through a ragged opening. It was quite clear that the upper or smaller opening in the front of the auricle, and which was surrounded with ecchymosis, marked the entrance of the ball. There was very little blood in the pericardium, not more than an ounce ; but in the right cavity of the pleura there was a large quantity of blood, and it was found that the ball had taken an oblique course through the lower lobe of the right lung. They could see on the posterior surface near the edge, the opening through which the ball had come out, tearing considerably the soft spongy substance of the lung. It had gone on still further ; it had broken the ninth rib at its lower part, and gone out beneath the skin, which, however, it had not broken.

He thought it very likely that, as the ball broke the lower edge of the rib, it cut across the intercostal artery ; and that the great quantity of blood effused arose as much from that cause as from the wound in the lung. The ball was found under the skin, at the back of the chest, and was nearly unaltered, only a little flattened where it impinged against the rib ; it was an ordinary revolver bullet. On examining the wound in the upper part of the thigh, it was found that the ball had gone down to the femur rather obliquely, had torn off a portion of the periosteum, and had roughened the surface of the bone a good deal, but had not broken it, though it had struck with such violence against it that it was quite flattened. It was observed by the resident student that the pupil of the left eye was contracted, and the right pupil was a little dilated. He could not say that he could give any satisfactory explanation of this. It was just possible that when the heart, which was so largely supplied by the sympathetic nerve, was torn in such a manner, the irritation extended up the great sympathetic to the ophthalmic ganglion, which sends branches to the iris ; but why one should be dilated, and the other contracted, he could not undertake to explain. Another curious fact was that, although there was such a very large rent and free opening, there was so very little bleeding—there were not more than two table-spoonsful altogether in the cavity of the pericardium ; it might be that the wound of the right auricle interfered with the further reception of blood into the heart, and only parted through the wound with the blood absolutely in the cavity of the ventricle at the time. Death, therefore, did not ensue from the loss of blood arising from the wound of the heart. Mr. Hamilton then exhibited a drawing of a case which his colleague Mr. Adams had brought before the Society many years ago. A man was going across the street, and a gun was discharged by accident, lodging a quantity of slugs in different parts of his body. One of them penetrated under the left nipple. The man instantly ran across the street to an apothecaries' shop, and said he was seriously hurt. He was at once brought to the Richmond Hospital, and in half an hour after receiving the wound he died.

In that case the hemorrhage was excessive. The blood escaped freely from the heart into the pericardium, and caused it to become so enormously distended that it pressed on the heart, and obliterated its action. Another curious fact in the present case was this :—Here was a man who, after the infliction of a large gaping wound of the heart, was able to run across the street. But if they searched the earlier records of surgery—the works of Paré, for instance—they would find many cases narrated of people, after receiving wounds of the heart, living for some time. Dupuytren, whose paper on the subject was most interesting, had recorded cases in which death did not take place for several days after the infliction of the injury; in one instance the patient survived for a week.—*February 26, 1870.*

Cirrhosis of the Kidney.—DR. FINNEY said that in the case he was about to bring under the notice of the Society three distinct pathological changes had occurred, which could, with probability, be shown to follow in consecutive order. The specimens had been taken that day from the body of a man, who, last November, had been in the Meath Hospital, under the care of Dr. Stokes, suffering from an attack of hemiplegia of the right side—a slight distortion of the face, and some want of power of motion of the right side of the body, being the chief symptoms he then presented. There was also some anasarca, and the urine was albuminous. After being in the hospital two months he left it, and in the beginning of January was admitted into the hospital of the South Union. Six weeks ago, he was so much recovered as to be able to resume his occupation, as a cab driver; and he carried on his trade until the night before last, when he was brought to the Meath Hospital in an unconscious and apoplectic state, both sides of the body being paralysed, while the face was drawn to the left.

He died on the following night. The only remarkable symptom he presented during this period was, that he had a full and bounding pulse.

On opening the abdomen, a great amount of fat was found in the omentum, and the kidneys were found thickly covered with fat. On laying them open, both were found diseased. In the right, the cortical substance was of much less extent than natural, and the pelvis of the organ contained several renal calculi. The other kidney was in a more advanced state of disease; it was converted into a cyst, and very little of the gland substance remained; but from its small size, and puckered surface, it was most probable that it had also previously undergone degeneration. The cause of the commencing hydronephrosis was plainly the impaction of a calculus discovered in the ureter, about three inches from the kidney. When the thorax was opened, a quantity of fat was found on the surface of the heart, particularly on the right side. The

left ventricle was hypertrophied, without any enlargement or diminution of its cavity. There was no valvular disease, except a slight thickening at one of the mitral valves. It was, in fact, an example of simple hypertrophy of the heart. The surface of the brain was slightly congested; and, on close examination, a pink spot was found in the fissure of Sylvius, and on laying open the brain, a large clot was discovered in the left ventricle. The brain throughout was rather softer than normal.

He considered that this case was originally one of renal disease. In November last the state of the blood was such, owing to the cirrhotic condition of the kidneys, that the brain was ill supplied with blood, and he attributed the hemiplegia which then occurred to softening of a portion of the left side of the brain.

The condition of the heart was also to be accounted for in the same way, as it was well known that in advanced Bright's disease, the left ventricle of the heart was found hypertrophied, either due to the condition of the blood irritating the left ventricle, and giving rise to greater action, or owing to obstruction to the circulation through the kidney from the obliteration of the Malpighian bodies, which obtains to so great an extent in cirrhosis of the kidney.

The immediate cause of death was the hemorrhage into the brain, produced most probably by the strong action of the left ventricle upon the blood vessels of the brain, which, themselves degenerated, and losing the support which healthy brain tissue gives them, yielded and gave way in some spot.—*February 26, 1870.*

Hydronephrosis.—DR. FINNEY exhibited a specimen of hydronephrosis of the left kidney. It wanted, however, the interest of clinical history, having been taken from the body of an adult male subject in the adjoining dissecting room. Before opening the body, a tumour was distinctly felt in the left side of the abdomen, dull on percussion, and slightly fluctuating, which proved to be the cyst he exhibited. Its position was in the left lumbar region, extending from about an inch from the hypochondrium to the ilium. It had pressed over the intestines to the right side of the abdomen, and in front of it was the descending colon. Its weight was twenty-three ounces, and it contained a fluid very slightly albuminous. The vessels which supplied it were of great size. The ureter was impervious, no fluid passing through it when the kidney was pressed upon. The other kidney was much hypertrophied, weighing eight and a-half ounces, but was otherwise healthy. On examining the bladder and urethra, no cause was discovered which could account for the state of the kidney. To explain the cause of hydronephrosis was not always easy, as the conditions under which it was produced were very numerous. Causes of a congenital character were, perhaps, among the most frequent. A valvular arrangement of the opening of the ureter, which most commonly

was seen running along the posterior aspect of the kidney, and adherent to it: the plugging of the ureter by calculi or hydatids; inflammation of the ureter, causing its obliteration; or tumours pressing on it—any obstruction, in fact, to the flow of urine from the bladder, would be sufficient to cause hydronephrosis, partial or complete. In this case the probabilities were, that it was not altogether congenital; and the reason he thought this was, that the ureter in the rest of its course was of normal size and development, while the vessels going to the kidney were out of all proportion to the amount of renal tissue which remained. In the specimen, in fact, hardly a trace of glandular tissue could be found. It was converted into one large cyst, which after drying and inflation, was seen to contain a number of loculi, corresponding, most probably, to the fetal lobular compartments.

As to its clinical importance, it was impossible to diagnose it from the symptoms alone, as usually the general health was good, and but little of importance could be learned of the urine.

Its presence as a tumour was common enough, and it has been mistaken for ovarian and other tumours, and tapped repeatedly. Professor Dumreicher, of Vienna, tapped one of these tumours five times, and removed eighty-five quarts of fluid.—*February 26, 1870.*

Aneurism of the Aorta.—DR. FAMES exhibited a specimen, taken from the body of a man who died on Wednesday last, from the effects of aortic aneurism, in Mercer's Hospital. He was a man of temperate habits, and had enjoyed excellent health until about four months before his admission to hospital, when he began to suffer from cough and difficulty of breathing. One month ago, after a severe fit of coughing, he spat up mucous streaked with blood. He complained of intense pain in the lower part of the sternum, and also at the seventh dorsal vertebra. The carotid, brachial, and radial arteries were pulsating visibly, as in cases of aortic regurgitation. At each cough the veins of the neck became greatly distended. There were two areas of pulsation visible—one under the left nipple, the other to the right of the sternum. There was no prominence anywhere of the walls of the chest.

Percussion revealed continuous dullness from outside the left nipple to an inch to the right of the sternum. Hepatic dullness reached nearly to the umbilicus. There was a want of resonance, rather than actual dullness, across the sternum, at the level of the second rib; no pulsation could be detected in this situation.

On auscultation, he found a loud murmur to the right of the ensiform cartilage, which became less distinct as the stethoscope was carried upwards. On a level with the third costal cartilage, a double murmur was audible, quite distinct from the murmur heard below. At the second right cartilage, the murmur was single, and diastolic in rhythm. No

murmur could be heard below the left nipple; nor was there any along the spine till the stethoscope was placed beside the seventh vertebra; then a faint murmur was heard. The patient complained of a boring pain, not increased by pressure, in this situation, and further that his food always seemed to stop there. There was tracheal stridor audible between the scapulæ. Respiration was equal in both lungs, and *râles* existed at the base of the right lung. The pupils were of equal size. He used to sit up in bed crouching forwards, saying that the pain and dysphagia were relieved by this posture.

Matters remained much the same until Wednesday morning, when he was seized with hemoptysis, spat up a very large quantity of blood, and died.

Some hours after death *autopsy* was made. When the sternum was removed, the appearance presented by the parts lying in *situ* was as if there were two hearts side by side. The heart was pushed aside, and an aneurismal pouch partially occupied its place. In the portion of the sternum overlying this pouch was found a deep erosion, the seat of the pain complained of during life. This aneurismal pouch was found to spring from the right sinus of Valsalva; it passed directly downwards. The liver was greatly depressed; it was congested, but not enlarged (bile was found during life in the urine). But we had not yet found the source of the hemoptysis. On slitting down the trachea, a large irregular aperture was seen. A probe directed through this passed into the aorta, where we found a sacculus developed on another aneurism, which last was of considerable size; and on further examining the aorta, another aneurism still was found at the juncture of the ascending and transverse portions of the vessels. The remainder of the aorta was free from disease. The œsophagus had not been pressed upon, nor had the vertebræ. The stomach was full of blood. The orifices of the heart and the valves were perfectly healthy. Hence the regurgitation, which certainly existed during life, must have been due to some alteration in the orifice produced by lateral pressure, interfering with the action of the valves. The systolic murmur was caused, no doubt, by the entry of the blood into the orifice of the descending sac.

The tendency of aneurismal pouches springing from the sinus of Valsalva, or very commencement of the aorta, to enlarge downwards was noticed in 1836, by Professor R. W. Smith,^a who ascribes it to the circumstance of the opening into the sac being placed within the influence of the retrograde flow of blood.—*February 26, 1870.*

Strumous Disease of the Urinary Organs; Small Bladder.—DR. BENNETT exhibited the urinary organs of a boy, aged fifteen, of a highly strumous

^a Dublin Medical Journal, Vol. ix., page 425.

aspect, who had died lately in the Richmond Hospital, where he had been for some months, under the care of Professor R. W. Smith. He had previously been for some time in Jervis-street Hospital, where he was circumcised for the relief of congenital phymosis. He had suffered for years from incontinence of urine, and the examination of the bladder with a sound failed to detect any calculus, or any diseased condition except that of a very much contracted bladder. On his admission to the Richmond Hospital, the incontinence was complete—the urine unceasingly dropping from the orifice of the urethra. Under treatment he was occasionally able to retain the urine for three or four minutes, never longer.

About a fortnight before his death, a fluctuating and painful tumour, manifestly an abscess, was detected in the right lumbar region, and could also be felt in front, below the edge of the liver. The boy was greatly emaciated, and finally sank, worn out by the distress induced by the incontinence of urine, and the development of the abscess in the lumbar region. There existed, from the time of his admission, an unhealthy ulceration around the orifice of the urethra. He died February 12, five months after his admission into the hospital.

On examination after death, the left kidney was found to be completely degenerated. It was converted into a multilocular cyst, filled with a curdy fluid. The ureter was occluded at the pelvis, and again for some distance above the bladder—a short contracted portion of it near its centre being patent; much difficulty was found in tracing the ureter to its termination in the bladder, as it was involved in a portion of the pelvic areolar tissue, which, while it had matted the parts firmly together, had probably led to the occlusion of the tube; no trace of the opening of the ureter into the bladder could be detected. All the appearances seen on the left side indicated that the disease of the kidney and ureter had existed for a long period. On the right side the kidney was found to be in a state of acute scrofulous degeneration; the greater part of its tissue was studded with scrofulous abscesses of various size, the entire gland being considerably enlarged. In the areolar tissue behind the kidney there existed a large abscess, which did not communicate with the gland or with the ureter. The ureter was much dilated, and a probe could be passed along it directly into the bladder, through an opening of considerable size. The usual oblique passage through the coats of the bladder seemed to be absent, or obliterated by the distention of the tube. The most remarkable portion of the specimen was the extremely small size of the bladder, and the undeveloped condition of its walls. So minute was the organ that at first no trace of it could be found. On making a section in the middle line, a cavity was opened which would contain a large sized pea; the walls of this cavity were membranous, except where they were related to the indurated tissue in the vicinity of the obliterated left ureter. Into

this cavity the right ureter opened. The neck of the bladder was defined by a slight constriction, which separated the cavity from that of the prostatic portion of the urethra, which exceeded it in dimensions. There existed in the floor of the prostatic portion of the urethra, immediately outside this constriction, a circular depression, which was apparently the cicatrix of an old ulcer; this destroyed the usual crest that divides the prostatic sinus, where it should pass into the *uvula vesicæ*. On passing fine wires along the vasa deferentia, they were found to open into this depression; but the wires passed onwards, entered again into a nipple-like projection, which existed on its anterior border, and issued finally at the summit of the projection; across the depression a fine bridle of mucous membrane passed.

From these facts it was apparent that an ulcer had existed immediately outside the neck of the bladder, and that it had opened both vasa deferentia, a short distance before their termination in the urethra. There did not exist anything worthy of note in the anterior portions of the urethra. The lungs were in an advanced stage of tubercular disease. The history of the case is defective, nothing being known exactly of the length of time during which incontinence of urine existed, beyond the fact that it had existed for more than five years. The anatomical examination shows that scrofulous disease of the kidneys, ureter, and probably of the urethra, existed for a long time; that the bladder was of the smallest possible dimensions, and that its muscular coat was completely undeveloped. Whether the condition of the bladder was congenital, or a consequence of scrofulous disease of the parts occurring in early life, it is difficult to decide. It seems, however, most probable that congenital deformity existed, and that scrofulous disease attacked first the left kidney, ureter, and urethra, and subsequently the right kidney.—*March 5, 1870.*

Tubercular Disease of the Urinary Organs.—DR. T. E. LITTLE exhibited a specimen, consisting of the urinary and genital organs, taken from a body which had been brought for dissection into the Dublin University Anatomy Room. It consequently possessed the disadvantage of being unaccompanied by any clinical details.

The body, from which the organs before the meeting—the kidneys, ureters, bladder, urethra, and uterus—were taken, was that of a woman, aged apparently about forty-five; and it presented (for a dissection room body) an unusually healthy and well-nourished appearance; no œdema of any part existed.

The parts exhibited gave the whole sum of disease discoverable in the body, all the other organs presenting to the eye appearances of perfect healthiness.

No urine remained in the bladder after death, so that the condition of that secretion could not be reported on.

The two kidneys presented such very different conditions as to necessitate their description separately. The *left kidney* was so invaded by disease that, on a rough calculation, not more than one-sixth of its glandular structure remained. The parts so intact lay to the inferior and outer part of the organ, and appeared both to the eye, and under the microscope, to be healthy. The remainder of the gland was occupied by numerous masses of yellow, cheesy matter, of various sizes. These in many—indeed, in most—places had undergone a process of softening, leaving veritable vomicæ, which had soft, irregular, yellowish walls, and had communicated with, and evacuated their contents into, the infundibula in several cases. They were surrounded each by a narrow zone of congested gland tissue. The tubercular nature of these seemed to be clearly indicated by the appearances presented in the infundibula, when opened up—viz., they were studded over in many parts with numbers of small whitish tubercles. These dissection proved to be situated in the submucous tissue; the mucous membrane was capable of ready dissection from off them, and perpendicular microscopic sections showed them to be altogether beneath the layer of epithelium. These diseased conditions affected the medullary decidedly more than the cortical regions. The pelvis of this kidney was healthy, as was also the corresponding ureter.

The *right kidney* was of very irregular shape, was slightly enlarged, and nodulated. When cut into, no trace whatever of healthy gland tissue was found to remain—the whole organ appearing as though made up of a number of cysts, separated from one another by strong walls of condensed fibrous tissue. The majority of these cysts communicated with the pelvis, though some did not do so. They were of large size—one was as large as an egg, the others averaging the size of an ordinary walnut. They were all filled with a putty-like matter, of considerable consistence. Their contents, in fact, proved to be composed of inspissated pus—the pus corpuscles remaining singularly little unchanged, and had throughout small masses of amorphous mineral matter, which dissolved, with effervescence, in nitric acid. The pelvis of this kidney was greatly dilated, and its walls much thickened, so that it exactly resembled the cystic cavities in the kidney, and like them was filled with contents of the characters described. The ureter generally was much constricted in calibre, and thickened in its walls; in most places it scarcely admitted the introduction of a small probe, but was still pervious throughout, and communicated both with the pelvis of the kidney and with the bladder. In one or two places it was dilated into spindle-shaped, aneurism-like expansions, and through its whole course it was filled with the same putty-like material.

The bladder was remarkable, chiefly for its extraordinarily small size; as removed from the body, its cavity possessed a capacity not greater than that sufficient to hold a moderately large walnut. Its coats were

of normal thickness. The mucous membrane was healthy, except in two places—that is, at the opening of the right ureter, and near the urethral orifice, in which regions it was roughened, discoloured, and ulcerated; and where several small submucous tubercles, similar to those in the renal infundibula, could be detected.

The urethra—strongly contrasting in this with the condition of the bladder—was of enormous size, freely admitting the introduction of two fingers; its mucous membrane was much ulcerated.

The uterus and its appendages were healthy.

Remarks.—The evidence for the originally tubercular nature of the affection described, has been alluded to in noticing the condition of the infundibula of the left kidney, the whitish tubercles in the submucous tissue of which, when examined microscopically, presented characteristic appearances. It must be confessed that, but for the co-existence of the state of things found in this kidney, we might have some difficulty in assigning its true pathological signification to the condition of the other. The latter peculiarities—resembling so much those met with as the result of simple chronic pyelitis—would seem to be explained by the fact that, in this instance, the tubercular change appears primarily and of preference to have affected the mucous membrane of the excretory part, rather than the secreting structures of the organs.

As to the absence of tubercle from the other parts of the body, so little unusual is this, that most pathologists have classified tubercular disease, in its relations to the urinary organs, into (*a*) that in which the lesion of the urinary system occurs as an isolated fact, and (*b*) that in which it is a part of a general manifestation of disease throughout the body.

The immunity from disease enjoyed in this case by the genital organs is in accordance with the strange and unexplained pathological law, that whereas in the female, tubercular disease of the urinary and genital organs occurs usually in each of these systems independently and unconnectedly; in the male, on the contrary, the disease most frequently implicates both of them simultaneously. I may mention that this law was still further exemplified, in the latter part of its statement, in the case of another body (of which I have a note) met with in our dissecting room during this session, in which, in a male subject, along with extensive tubercular disease of one kidney, the ureters, and bladder, extensive disorganization of the vesiculæ seminales, and abscess of the prostate, were found.

The significance of the extremely small size of the bladder is much enhanced by the coincidence of the exhibition by Dr. Bennett, for Professor Smith, at the same meeting of the Society, of the morbid parts, in a case of remarkably similar disease of the urinary organs; in which case also *post mortem* investigation demonstrated the presence of a bladder of

still more diminutive proportions. Might not this pathological fact have a connexion with the great frequency of micturition so constantly present, and so well known as a symptom of tubercle of the kidney?

The continuance of life, with such a small amount of urinary secreting tissue as remained in this case, must strike us as remarkable. It is impossible to say how far the urinary ailments were the cause of death, as inquiry failed to arrive at anything of the antecedent history of the subject.—*March 5, 1870.*

Pneumonia.—DR. WHITE said, the specimen which he exhibited was one taken from the body of a man, aged thirty-four years, who died suddenly in the street, on the 5th of the present month. The body was brought to the Meath Hospital.

On opening the thorax, the lungs did not collapse, and it was found there was almost complete adhesion between the costal pleura and that investing the lung. The entire pleural cavity was obliterated, except in one or two small spots. The left lung was considerably smaller than the right. After it was removed from the chest, it collapsed somewhat; and, on cutting into it, the appearance presented was that of a lung in the second stage of pneumonia. The right lung was in a state of complete carnification. There was no crepitation to be detected in any portion of the organ, nor any evidence of tubercle.

On opening the pericardium, the external appearances of the heart were normal. Both sides of the organ contained large clots, which extended into the aorta and pulmonary arteries. They were not able to obtain an accurate history of the case.

The man had been a car driver, and had been complaining for a fortnight before his death. He never gave up work, but dropped dead in the street. During the fortnight, he suffered chiefly from oppression about the heart, and shortness of breathing. He had not applied for medical advice.—*March 12, 1870.*

Congenital Luxation of the Clavicle.—PROFESSOR R. W. SMITH brought under the notice of the Society an interesting specimen of congenital dislocation of the acromial extremity of the clavicle, and expressed his great regret that he was unable to accompany the description of it by a history of the case during life, as the gentleman who gave him the preparation was not acquainted with any of the commemorative circumstances connected with it.

With respect to the scapula, judging from its size, it seemed to have belonged to the skeleton of a female. There was a complete absence of an articular surface upon the acromion; there was not the slightest trace of its having ever existed. The acromion terminated in a thin, scabrous, and irregular margin; and its surfaces, instead of looking upwards and

downwards, were directed inwards and outwards, its subcutaneous surface lying in a plane parallel to that of the dorsum of the scapula; the coracoid process and the glenoid cavity presented no abnormal appearance worthy of notice.

There was a remarkable arrest of development of the clavicle, affecting all its diameters, but more especially its length, which amounted to only three inches and a-half; its outer extremity was curved downwards, so much so as to form nearly a right angle with the shaft of the bone. The outer surface of this curved portion (which resembled somewhat a coracoid process) was in contact with the inner surface of the spine of the scapula, more than two inches from the extremity of the acromion, the former process being superficially excavated where the clavicle rested against and moved upon it. The surfaces of the clavicle did not present their normal direction; that naturally the superior now looking forwards, the inferior being directed backwards; the normal anterior margin or surface, now the inferior, rested obliquely upon the posterior part of the coracoid process, upon which it seemed to have moved freely, being marked by a deep excavation of considerable length.

Although the history of this remarkable specimen is unknown, what may be termed the *circumstantial* evidence may be looked upon as being sufficiently strong to prove the congenital nature of the deformity. The chief features constituting this evidence are the following:—

1. The diminished length of the clavicle.
2. The change in the aspect of its surfaces.
3. The absence of an articular surface upon its acromial extremity.
4. The curved form of that extremity.
5. The want of an articular surface upon the acromion.
6. The abnormal aspect of the surfaces of the acromion.

It would have been interesting to have known the condition of the coraco-clavicular ligaments, but (if they ever existed) they had been destroyed by maceration. The appearances of the bones, however, would lead to the supposition that they were originally deficient; for no trace existed of the impression which marks the position of their attachment to the clavicle.

Professor Smith observed that he had carefully looked into the literature of the subject, and had been unable to find any record of an example of congenital luxation of the scapular extremity of the clavicle similar to that which he had just described.

Guérin^a has stated that he found it lying upon the upper surface of the acromion process in a fœtus of three months. Here the position of the bone was quite different from that which it occupies in the specimen on the table. Moreover, M. Giraldés has raised serious doubts as to the reality of this (as Malgaigne has termed it) microscopic luxation.

^a “Recherches sur les Luxations Congénitales.”

M. Martin, of Bordeaux, recorded, in 1765,^a a case which Malgaigne considers to be an example of congenital luxation of the clavicle. It was that of a man, thirty years of age, whose father and brothers were affected with a similar deformity. The clavicle was one-fourth shorter than that of the opposite side; its outer extremity, which was salient and rounded, was separated from the acromion process by an interval of two fingers' breadth. Its lower surface, which could be distinctly felt, presented to the touch no trace of the tuberosity which gives attachment to the coraco-clavicular ligaments. These structures seemed to be replaced by an osseous projection, which sprang from the coracoid process, and ascended towards the clavicle. All its normal functions were enjoyed by the shoulder.

It is, in my opinion, manifest that this case was not an example of congenital luxation, but rather of an arrest of development of the outer end of the clavicle.—*March 12, 1870.*

Scirrhus of the Pancreas.—DR. E. HAMILTON said that the preparation which he now exhibited had been taken from the body of a member of the Royal Irish Constabulary, fifty-six years of age, who was admitted to Steevens' Hospital, in the latter end of January, suffering from symptoms of jaundice in a marked degree. The skin was of an intense orange-yellow colour; the conjunctivæ were also yellowish, and the urine presented the characteristic colour. He complained of irritability of stomach, and of a peculiar itching of the skin, and he said that everything he saw had a marked yellowish colour. The history he gave of his case was, that he got this attack of jaundice about four months previously, being thrown into a state of great mental anxiety from the illness of his wife. A careful inquiry into his history elicited the fact that he had before this suffered from severe attacks of dyspepsia. On making an examination, there was detected, deeply seated in the right hypochondriac region, and extending somewhat towards the umbilical region, a tumour, very firm and solid, but by no means accurately defined, and at this point he suffered a considerable amount of pain, which was much increased by pressure.

The extent of hepatic dulness was not abnormal, nor was there any indication of increased size of the spleen, and pressure over the stomach did not cause any distress. The evacuations from the bowels presented the ordinary characters of jaundice, but there was no indication of the fatty discharges said to be indicative of pancreatic disease. Palliative treatment was adopted, but with little effect; and about a week before he died, he manifested some derangement of intellect; and two days before death he had, for the first time, very profuse *melæna*.

^a "Journal de Médecine de Vandermonde," t. xxiii.

The diagnosis was attended with considerable difficulty; and the nearest approach he could make to it was, that there was a tumour somewhere in the vicinity of the common duct, pressing on the gall bladder, and thus preventing the escape of bile; that the jaundice was, therefore, a jaundice of obstruction.

The symptoms were explained very satisfactorily by the appearances disclosed after death. On opening the cavity of the abdomen, a considerable quantity of yellow serum was found in the peritoneal sac. Passing over the liver, they were struck by the enormous size of the gall bladder, evidently arising from obstruction to the common duct. The head of the pancreas, where it was embraced by the curve of the duodenum, was converted into a dense hard structure, which cut like cartilage. There was no ulceration of the duodenum, and he accounted for the discharge of blood from the bowels by the obstruction to the circulation through the superior mesenteric vein, the greatest obstruction being where the vessel crossed the duodenum. The pancreatic duct was enlarged, dilated, and varicose, and it was by opening one of these enlargements that he was enabled to pass the probe into the pancreatic duct, so as to enter into the duodenum.

Dr. Hamilton observed, that the absence of fatty evacuations, in this case, materially increased the difficulty of diagnosis, the nearest approach to which he could make was, that there was a tumour in the immediate neighbourhood of the common biliary duct. He thought if they had prolonged dyspepsia, without organic disease of the stomach; if they had intense jaundice, caused by obstruction; if the liver and spleen were in their normal condition; and if there was late, in the disease, the appearance of melena, a chain of evidence would be made up, showing that the disease was in the pancreas; and, of course, if they had in addition the fatty evacuations, the point would be no longer doubtful.—*March 19, 1870.*

Caries of the Spine.—DR. GORDON presented a specimen of the caries of the vertebræ, taken from the body of a man who had suffered from the disease for upwards of four years. The case was remarkable for the suddenness with which paralysis occurred. The man, when admitted into hospital, was able to go about the ward, although the limbs were feeble, when, one day, as he lay in bed, he became conscious of something having given way in his back. Complete paralysis of motion and sensation of the lower half of the body ensued immediately, and he complained particularly of a great and general sensation of burning; an immense increase of temperature, in fact, took place. He became rapidly worse, and all parts at all exposed to pressure fell into gangrene. Three days before death there was an access of fever, and several small purulent deposits occurred over the wrists and hands. In fact, purulent absorption from the eschars had taken place.

Several of the dorsal vertebræ were engaged in the disease, and in the centre, the carious portion of the column had given way; the spinal cord was compressed to such a degree that a solution of continuity, which may have been said to have been complete, had taken place.—*March 19, 1870.*

Edema of the Lungs; Horse-shoe Kidney.—DR. GORDON exhibited a case of malformation of the kidney in a child, about four or five years of age, who had been admitted into the Whitworth Hospital a fortnight ago, suffering from general anasarca. It was assumed that this had arisen from scarlatina, now so prevalent in this city; and this turned out to be the case. The form of scarlatina was exceedingly mild; the boy was scarcely confined to bed at all; but in proportion as the scarlatina was mild, the anasarca was severe, very rapid, and extensive; and he was quickly seized with a severe attack of pneumonia. It was, perhaps, more properly speaking, an attack of intense œdema of the lungs; for the symptoms by which it was preceded were those of that peculiar form of hemoptysis which is accompanied with extreme dyspnœa and exceedingly severe cough, the blood coming up in profuse quantity.

On *post mortem* examination, it appeared that this bleeding was not from any particular part of the lung affected with an ordinary pneumonic inflammation, but that it was general throughout both lungs, which were everywhere the seat of the most extreme œdema. As was often the case in such an affection, particularly in young subjects, he succumbed very rapidly. So far as the œdema of the lungs was concerned, the entire duration of the disease was not more than two days; but during that time he had not been subject to any treatment, as he died very shortly after his admission into the hospital. Whether the condition of the kidney had anything to do with the rapid progress of the disease, he could not say. He did not himself think it was likely that it had; but on examining the kidney, it was found to be in a highly congested condition, and also malformed. It was quite clear that it was an instance of the blending of the two kidneys into each other. They could see the two ureters. The right kidney came down as far as the fifth lumbar vertebra.

Another peculiarity was, that the two supra-renal capsules were perfect, and fully developed. Cruveilhier has remarked, that in some cases of this malformation, the supra-renal capsules were larger than ordinary. In this case they certainly were not smaller, and both were in their normal positions.—*March 19, 1870.*

Apoplexy.—DR. WHITE said, that the patient from whose body the specimen before the Society was taken was a man, thirty-three years of age, who came into the Meath Hospital on Sunday, affected with epistaxis. He had been suffering for several days previously, and was bleeding freely



PROFESSOR R. W. SMITH — CONGENITAL LUXATION OF THE CLAVICLE.

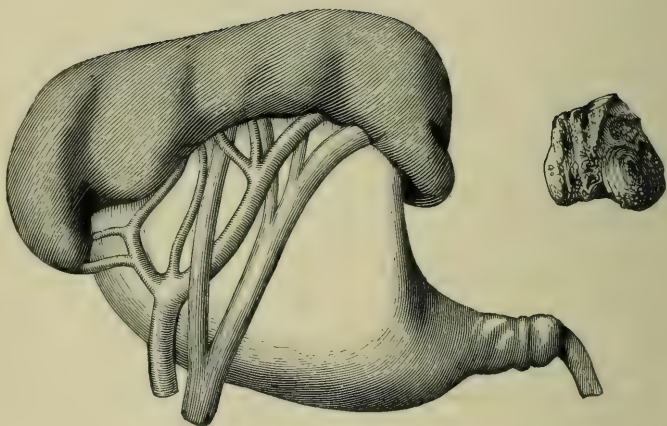
from the left nostril when admitted. The nostril was plugged anteriorly. The hemorrhage was quite checked, and the man appeared to go on well. In the evening, when he dropped asleep, the resident pupil observed that his rest was very uneasy. He was talking incoherently when asleep, and even when he was awake he sometimes spoke in the same way. He passed through the night, however, tolerably well. At four o'clock in the morning, he got up to go to the water-closet, and fell down in a kind of fit; and when put back into his bed, his breathing was observed to be somewhat stertorous. He remained in that state till half-past ten in the forenoon, when he died. When he (Mr. White), saw him in the morning, he was lying perfectly unconscious, with contracted pupils, and all his limbs paralysed. His breathing had then more of a tracheal character than that of actual stertor. On examining the surface of the body, he found he was covered with a syphilitic eruption of a tertiary character; there were also some cicatrices of former ulcers. On the following morning the brain was removed from the body. There was nothing very remarkable in the excess of serum; but, on pressing upon the pons, blood exuded; and on cutting into it a large clot, extending into the anterior portion of the cerebellum, was found in it.

On cutting into the brain, there was a large amount of serum found in the ventricles, but no coagulum. There seemed to have been some lymph effused under the arachnoid, but whether it was the result of syphilitic disease or not it was difficult to say. It was one of the severest forms of apoplexy, and one that terminated life more rapidly than almost any other. The question arose, was it judicious to stop the bleeding from the nares? This, of course, would not have been done had there been anything to lead to the suspicion that the man was about to have an attack of apoplexy.
—*March 26, 1870.*

Fatty Degeneration of the Heart.—DR. BENNETT exhibited the heart of a patient who had been for some time under the care of Professor M'Dowel, in the Whitworth Hospital. The patient was a woman, 23 or 24 years of age. She had suffered from cholera during the epidemic which occurred in Dublin in the year 1866, and dated the first symptoms of cardiac disease from the time of her recovery from the attack of that disease. The earliest symptoms were palpitations and distress in breathing, at first slight, but ultimately such as to make her seek for admission into hospital about two months before her death. Treatment in hospital failed to check the progress of the disease; the patient became dropsical, and suffered from great congestion of the veins of the head and neck, and orthopnea. She presented many of the symptoms of ordinary disease of the mitral orifice; but Professor M'Dowel observed that during the entire course of the case there existed no marked pulmonary congestion, nor any sign of pulmonary hemorrhage. A murmur at the apex of the

heart could be occasionally heard, more distinctly at times when the respiratory distress was relieved, but it was very frequently absent. When present, the murmur was heard most clearly at the right apex, and was lost in tracing it to the left side, towards the usual seat of murmurs developed in the mitral orifice. Professor M'Dowel inclined to the opinion that it was to be referred to the tricuspid rather than the mitral orifice; he hesitated to make a positive diagnosis, in consequence of the uncertain character of the murmur. The absence of pulmonary congestion he considered to be strong evidence in favour of the opinion that it was a tricuspid murmur.

On examination after death, the heart presented, at first sight, no marked deviation from health. It was not hypertrophied, nor wasted, and was free from any excessive deposit of fat on its surface. On section, the thickness of the walls of the chambers appeared normal, and the various openings were found free from disease. The aortic orifice, tested by a column of water, was found perfect; there were slight traces of disease in the inner tunics of the aorta, apparently inflammatory, but the valves were free from disease. The only deviation from health discoverable in the heart by the naked eye were more or less distinct markings of the lining membrane of the ventricles, and of the muscular tissue. In the left ventricle the endocardium was opaque, whitish, and apparently thickened in parts, in others it was normal; and the muscular tissue was seen through it, affected by irregular marks, which at first suggested the presence of trichinæ in the tissue. The opacities and the markings of the muscular tissue were both most abundant in the left ventricle; few, if any, opacities occurred in the lining of the right ventricle, and the markings of the muscular tissue were less confluent than in the left; they were arranged in waving lines, at right angles to the direction of the muscular fibres, and their arrangement suggested a condition of less complete development than that of the change produced in the left ventricle; a very careful examination of the muscoli papillares of the mitral and tricuspid valves showed that they were less rounded and full at their apices than normal, the muscular tissue being shrunk, and apparently in part replaced by fibrous tissue. The more opaque parts of the left ventricle showed, on section, a change of the same kind; the opaque endocardium seemed to be connected to a layer of muscular structure, which had been converted into fibrous tissue, and which shrunk when cut, so as to render the surface of a clean vertical section irregular; under this layer, the muscular tissue was seen to present the markings as in the parts covered by normal endocardium. The microscopic examination showed that the markings of the muscoli were due to localized deposits of oil globules in its tissue within the sarcolemma. The disease of the tissue differed from ordinary fatty degeneration, in its being localized to the adjoining parts of several fibres, not extending



DR. BENNETT.—CALCULUS IN THE URETER.

to any great distance in each fibre, and in the extreme degree of change produced within the limits of each patch of disease. Each muscular fibre could be found apparently free of disease between the successive patches.

To these facts were due the appearances seen by the naked eye, of waved markings, placed at right angles to the fibres—appearances not seen in ordinary fatty degeneration of the heart. The microscopic examination of the opaque parts of the left ventricle, and of the examination of the musculi papillares of the auriculo-ventricular openings, showed that in these parts the change of tissue had been greater than in the parts described. The alteration into fibrous tissue seemed to be the result of the complete wasting of the myoline, and the removal both of it and of the results of its degeneration. In these parts nothing remained of the muscular tissue but the wasted sheaths of the myoline and their connecting fibrous tissue. The muscular fibres could in parts be traced into these wasted patches, and they lost in them all their characteristic markings. Traced from the patches, they were found to pass into those of fatty degeneration. It was difficult to resist the conclusion that the fibrous change was the sequel of the fatty degeneration, and that the complete removal of the myoline had been preceded by its conversion into oil. Whether this be the correct explanation of the pathological changes or not, the examination proves that there existed in the heart, structural changes sufficient to incapacitate the muscular tissue connected with the valves of the auriculo-ventricular openings from retaining those valves in proper position during the contraction of the walls of the cavities. The variability of the physical signs, and their uncertain character, would appear to be due to the impaired contractile power of the entire muscular tissue.—*March 26, 1870.*

Renal Calculus.—DR. BENNETT exhibited a preparation and cast of a kidney and ureter, taken from the body of a male subject in the dissecting-room of Trinity College. The pelvis of the kidney and the upper portion of the ureter were greatly dilated, the latter measuring more than four inches in width at its point of emergence from the gland. The tissue of the gland was expanded over the distended pelvis and infundibula, but seemed in other respects healthy. There existed in the posterior part of its tissue a small cyst, unconnected with the pelvis, which contained a clear serous fluid. The fluid enclosed in the dilated ureter and pelvis was urine, containing a small amount of ordinary lithates. About five inches from the kidney the ureter was obstructed by a calculus, which completely prevented the escape of urine; but air could be caused to pass in the opposite direction from the lower part of the ureter into its upper dilated portion. On laying open the ureter above the seat of impaction, its mucous membrane and that of the kidney were found

free of ulceration or any sign of inflammatory action. The calculus was adherent to the membrane by a viscid mucus so closely, that it required a slight force to detach it after the tube was completely opened. On removing the stone and the mucous connecting it to the membrane, the surface of the latter was found free from ulceration. The calculus, which was composed of oxalate of lime, was twenty-seven grains in weight, and measured three-fourths of an inch in its longest axis. The only traces of inflammatory action in any part of the specimen were two very small abscesses in the areolar tissue investing the kidney. The remaining portions of the urinary apparatus were healthy.—*April 2, 1870.*

Calcified Bronchial Gland.—DR. FOOT exhibited a large calcified bronchial gland, taken from the posterior mediastinum of a woman, 25 years of age; it was shaped like an overgrown patella, was 2 inches and 4 lines long, 2 inches wide, $1\frac{1}{4}$ inch in thickness, and weighed $2\frac{3}{4}$ ounces. When sawn through, the central part was found to be rather spongy, the cretification having proceeded from without inwards; the colour of the interior was a dirty white; only one small vascular point appeared; when the finger was passed along the inside of the œsophagus, it was rendered evident that the tumour compressed that tube very perceptibly.

The woman attributed her illness to indigestion, and frequently complained of “a lump in her stomach.” Between the gland and the root of the right lung lay the right pneumogastric nerve, grooving the tumour; numerous small vomicæ existed in the upper portions of each lung, but nowhere in the lung did any attempt at cretification of the tubercle appear.

On analysis, the centesimal composition of the gland was as follows:—

Moisture,	10·88
Organic and Volatile Matter,	41·30
Phosphate of Lime and Magnesia,	10·09
Carbonate of Lime,	37·71
Loss, etc.,	00·02
		<hr/>
		100·00

The analysis was made a long time after the removal of the gland; the percentage of moisture was originally much more greater.—*April 9, 1870.*



DR. FOOT.—CALCIFIED BRONCHIAL GLAND.

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